LINGUISTIC (IR-)REALITIES.

A HEURISTIC CRITIQUE OF THE META-THEORETICAL FOUNDATIONS OF GENERATIVISM.

ANDREW CHARLES DAVIDSON

The School of Oriental and African Studies,
The University of London,
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ProQuest LLC.
789 East Eisenhower Parkway
P.O. Box 1346
Ann Arbor, MI 48106 – 1346

ABSTRACT.

The thesis aims to provide a heuristic critique of the meta-theoretical foundations of Chomsky's project for an explanatory linguistics. The critique is 'heuristic' in that it attempts to take the considerations adduced to indicate how those conceptual foundations are to be re-designed on lines parallel to constructivism in the philosophy of mathematics. The net result is the provision of an outline of a meta-theoretic rationale for a process orientated linguistic theory (e.g. Kempson *et al.*'s LDS_{NL} framework).

The thesis investigates, and is organized around, three central strands of the Chomskyan paradigm:

1) The mathematization of linguistics: the use of formal/mathematical systems as theory constitutive metaphors.

2) A scientific realist (as opposed to instrumentalist) construal of linguistic theories.

3) A conceptualist/psychologist ontology for linguistic objects with a concomitant explanation for the nature of the linguistic in terms of properties of the modularized human "mind/brain" articulated through a system of mental representations.

The central conclusions drawn are:

1) There is a failure to achieve adequate warrant for a scientific realist construal of Chomskyan linguistic theories.

2) The object(s) of study that is (are) posited in the Chomskyan paradigm require a Platonist or autonomist ontological status. A corollary of this is the inability to achieve an adequate explanation for the nature of linguistic phenomena.

These conclusions, together with the observation of certain conceptual tensions and antimonies in generativist thinking (e.g. the relation between types and tokens), are taken to be sufficient to prompt a re-examination of the (metaphysical realist) assumptions that underlie that thinking. The solution that is canvassed, and which promises to resolve these tensions, is by way of a linguistic version of mathematical constructivism in which the emphasis lies in linguistic phenomena being construed as primarily cognitive events in which the constructive procedures are crucially constitutive of their linguistically individuating properties.

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Chapter III: Ontologies for the Linguistic I: Non-Psychological Realism.

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Thanks are also in order to Bill Downes and Clive Matthews at the University of East Anglia who introduced me to linguistics and who showed me why, in Clive's phrase used as a mantra to charm first year undergraduates, linguistics is interesting. To a considerable extent it is Bill's enthusiasm and concern for philosophical issues that is reflected, (again one wishes it had been reflected better,) in the contents of the following.

A further debt is due to Annette, who over the past couple of years has had me, not always reasonably, to put up with.

INTRODUCTION.

> *Real progress comes not so much from collecting results and storing them away in 'manuals' as from inquiry into the ways in which each particular area [of inquiry] is basically constituted. ... The level which a science has reached is determined by how far it is capable of a crisis in its basic concepts.*
>
> (M. Heidegger, *Being and Time*, section 3, 9)

> *Non ergo grammaticus sed philosophus proprias naturas rerum diligenter considerans ... grammaticam invenit* [?]
> (Is it, therefore, not the grammarian but the philosopher, carefully considering the proper nature of things ..., who discovers grammar[?])[1r]

The presiding aim of this thesis is to attempt to clear the conceptual ground such as to provide a rationale for a process-orientated and constructivist[2] theoretical perspective on the linguistic. The base intuition to which the taking of such a perspective answers is that natural language has an ineliminably temporal and procedural dimension; that linguistic objects are only identifiable, take on their individuating properties and meet proper criteria for existence in psychological event. The suggestion is that such a temporal and procedural dimension is crucially constructive of the properties and identity of linguistic objects and so is involved in their understanding (in both senses of the ambiguity) and in their explanation. It will be a contention that this dimension is denied by Chomsky's originary positing of the object of study as a competence grammar, with its attendant denumerably infinite set of abstract and timeless sentence types, and to which performance factors are in a no more than contingent and supplementary relation. In very brief, Chomsky's object of study, as has been argued by Katz *et al.*, demands to be construed as having a Platonist (or, perhaps, following Carr, an autonomist) ontological status and hence embarrasses the attempt to ground an explanatory linguistics in terms of properties of the mind/brain. Katz' Platonism, however, is not a solution (and nor is Carr's autonomism) but constitutes the problem for it obstructs any attempt to explain the relation between the linguistic and the individual; in other words, exactly the

relationship that Chomsky set himself the task of elucidating by pitching his enquiry at the level of "individual psychology". Precisely the unpalatability of a Platonism is that it can only perpetually defer the epistemological question (posed for mathematics by McCulloch and here rewritten for the linguistic case: "What is [a language], that a man may know it, and a man, that he may know [a language]?"[3]) as to how a supposed set of always and already, mind-independent, abstract objects can come to be apprehended and known by fleshly denizens of this actual and time-ridden world. What is needed, we will suggest, is a reconceptualization of the object of study: a language is not so much something that we know as something that we do: in von Humboldt's words, "in itself language is ... an activity".[4r]

The way to grounding the need for such a change in theoretical perspective will be through a critique of the conceptual underpinnings and explanatory strategies of Chomsky's project for an explanatory and psychological linguistics. The point of this critique will be to bring to light certain inadequacies and conceptual difficulties attendant on the explanatory strategies and theoretical constructs posited within the Chomskyan paradigm. To this end we will take as our motto Chomsky's contention that:

> The critics task is to show some fundamental flaw in principle or defect in execution or to provide a different and preferable account.[5r]

In suggesting the need for an alternative conceptual foundation there is the onus on the proposer, on the basis of if it ain't broke don't fix it, to point out what precisely is broke. The hope will be that such a critique will constitute a heuristic so as to point toward and act as, to use a rather old-fashioned word, a prolegomenon to a more adequate solution.

It is somewhat contrary to fashion to privilege philosophical matters as opposed to the hard scientific issue of answerability to data. It is seemingly customary to assume, or to pretend, that the foundational problems of linguistics have been solved[6r] and that the interesting and important matters lie at some empirical coal face where at least some honest work is done rather than in the effete lounges of the philosophers.

One can readily provide a Kuhnian spin to this state of affairs: after Chomsky's revolution comes a period of ordinary science conducted on the basis of a

set of more or less shared assumptions, even if this shared basis is often no more than implicit and mediated by a common methodology and practice. Both the confidence exhibited and the corresponding lack of contemplation of foundational issues are reasonably based on the hard results that are forthcoming and which can be cited as evidence for the basic correctness of the underlying assumptions. Newmeyer tells a story consistent with this interpretation where he cites as justification for optimism about the generativist enterprise the fact that we know hugely more now, at the observational level, than we did just three or four decades ago, and this despite more than two thousand years of previous, reasonably well-organized enquiry.[7r]

The state of the field is not, however, uniformly considered to be an unqualified success. To some the present state of play exhibits no more than "intellectual bankruptcy".[8r] One ground for disquiet is, arguably, the bewildering plethora of competing frameworks (there are, to pick up on our prefatory quotation, so many different and mutually incompatible "manuals"), to which one may also add the oscillating universe that revolves around MIT where frameworks get a complete overhaul on average, it seems, every ten years or so. To some this diversity is a symptom of "health" and vibrancy, no more than an illustration of the under-determination of theory by data. However, it may equally well be construed as a symptom of fracture and chaos in which there are neither sufficient grounds evidenced by any one framework for it to be uniformly assented to, but nor, crucially ("crucially" if we are to have a Popperian view of scientific enquiry as conducted on the basis of disconfirmation of theory), are there sufficient grounds to discount it or any others.

To a certain, puritan turn of mind - mine - there is something deeply unsatisfactory about a situation in which there are so many stories and no prospect of closure; to that turn of mind the field might come to appear as hardly distinguishable from gratuitous and idle talk. What licences the proliferation of frameworks (a symptom of vibrancy or is it crisis?) is a seemingly radical inability to make some reasoned decision either for or against any particular framework on the basis of the available empirical data. Precisely the point of focussing on meta-theoretic considerations is that it promises, on the one hand, to supplement the apparently rampant under-determination of theory by empirical data, and, on the other, to address the issue of, in Chomsky's words, "the correctness of principles employed".[9] If the conceptual underpinnings, the paradigmatic assumptions, can be shown to be sound or to be flawed, one is adducing strong evidence *pro* or *con* any given theory.

Chomsky's revolution, we are told, instituted a properly explanatory and scientific linguistics: a grammar is not merely a taxonomic device for more or less efficiently characterizing linguistic phenomena, but is "a theory of language", which theory "constitutes an explanatory hypothesis about the form of language as such."[10r] In attempting an explanation, i.e. in relating a discrete set of phenomena to a cause, the received view, at least as received from Chomsky, is that linguistics is involved in an enterprise on a par with that of the natural sciences, in which, as noticed as far back as Aristotle, the mark of proper understanding, the mark of an explanation, is to know not only what is the case but also *why* it is the case. (Compare our amended quotation from McCulloch, above.) Arguably, it is the addressing of this latter question at all, just so much as the manner of its addressing, that distinguished Chomsky's enterprise as revolutionary in the intellectual context of linguistics in the fifties.[11]

An explanatory hypothesis involves the positing, defining and elucidation of a proper and discrete object of study. That object is defined, in the Chomskyan paradigm, as an internalized, mentally represented grammar which constitutes one's linguistic competence; this grammar, in effect a tuple reflecting a modularization of the gross phenomenon and including a syntax, phonology and semantics, is "what one knows when one knows a language".[12r] The explanation of the nature of the linguistic invokes this knowledge relation between a speaker and a grammar: this knowledge, as instantiated in the mind/brain of a speaker, is what is explanatory of the properties of linguistic objects; the reason why natural languages are as they are is because of the nature of the knowledge instantiated by the mind/brain substrate; the explanation is in terms of postulated properties of the mind/brain. It is this object of study, the competence grammar that is the central concern of Chomsky's generativist paradigm, irrespective of whether or not one is entirely happy with the grammar's putative ontological status as a psychological entity.

The aim of what follows is to examine the set of claims and explicit, and sometimes implicit, assumptions that ground this enterprise and to bring to question the reality of the constructs of competence-orientated, syntax privileged, linguistic theories. Our approach will be in the manner of a somewhat Anglo-Saxon style deconstruction of three central strands of Chomsky's revolution:[13]

1) The application of formal systems to the study of natural language and so the fulfilling of Bloomfield's project for the mathematization of linguistics ("to make our linguistics a kind of maths") first aired in the twenties.[14r]

2) The scientific realist construal of the linguist's grammar, which grammar is offered as a theory providing an explanation of the nature of natural language. This instances a move towards a "God's truth"/realist interpretation of the analyses of linguistic phenomena as opposed to a "hocus-pocus"/instrumentalist view.[15r] In other words, linguists get to attempt to say something true about language rather than simply invent classificatory and characterizing schemata: a theory "carries a truth claim if it is serious."[16r]

3) The move towards viewing language as a phenomenon for which an explanation is to be attempted and, crucially, to be attempted in terms of mind: mind gets reinstituted as a respectable scientific notion after its years in the Behaviourist wilderness and it is instituted on the basis of a concept of mental representation. This claim promises to enable a reduction of the linguistic to the psychological and this, in turn, promises to be reducible to the neurophysical and biological: linguistics is claimed to be a sub-branch of psychology, itself a sub-branch of biology. In positing a linguistic theory one is also positing a research program in psychology and the brain sciences in general.

The first strand enables the precise definition and articulation of an object of study, the second introduces a scientific realism in respect of that object of study together with a realist construal of posited theoretical constructs, and the third provides the object of study's, and the theoretical constructs', ontological characterization and enables an explanation that relates the linguistic to its putative causal substrate. These three strands, somewhat intertwined, provide the central themes, and organization, of our discussion.

Footnotes.

Throughout, where a footnote cites no more than a bibliographical reference we will indicate this by a superscripted "r" following the footnote number.

[1] Cited by Robins (1967, pg. 86) from a collection of mediaeval linguistic writings (Thurot (1867)).

[2] "Constructivist" is a term borrowed from the philosophy of mathematics (Bishop, (1967), this following on from Brouwer's intuitionism). The term has both methodological and ontological implications: the constructivist in mathematics is suspicious of completed infinite sets and, correlatively, of methods of proof that result in the positing of objects that are deemed to exist but which cannot be individuated; in other words, objects which cannot be definitively indicated: "when a man proves a positive integer to exist, he should show how to find it" (Bishop, 1967). Showing how to find it, i.e. some demonstration of how it is to be found/constructed, is criterial for the content of an existence claim. For an account of the issues and of the origins of the constructivist/intuitionist perspective see M. Kline, (1972 and 1980).

[3] W. McCulloch (1965), cited in S. Dehaene (1998, pg. 231). It is something of a mathematical counterpart to, and amplification of, Chomsky's (1986) question as to "what [it is] we know when we know a language?"

[4] W. von Humboldt (1970, pg. 27).

[5] N. Chomsky, (1980, pg. 12.)

[6] The point was made in a talk given by J. Katz at SOAS in October, 1994 and repeated by him more recently (J. Katz, 1996, pg. 277).

[7] F. Newmeyer (1983, pg. 49). The same point is expressed by Chomsky (1986, Chapter 1).

[8] N. L. Love (1988), cited in R. Harré and R. Harris (eds.) (1993, pg 14).

[9] Chomsky remarks (1980, pg. 11): "Substantial coverage of data is not a particularly significant result; it can be attained in many ways and the result is not very informative as to the correctness of the principles employed."

[10] N. Chomsky, (1965, pg. 27, and 1980, pg. 109).

[11] The anti-explanationism of much of pre-Chomskyan, fifties' linguistics is quite overt: "Anything in our description that sounds like explanation is simply loose talk." (M. Joos, 1957)

[12] N. Chomsky, (1986).

[13] Compare Chomsky's own retrospective sketch (1991, pg. 21).

[14] L. Bloomfield (1926).

[15] The phrases are F. Householder's (1952) in his review of Z. Harris (1951). The terms "realist" and "instrumentalist" will be provided with some definition below.

[16] N. Chomsky (1980, pg. 109).

CHAPTER I.

The Mathematization of Linguistics.

No one should be afraid that the contemplation of characters will distract us from things, on the contrary it will take us to their very heart.
(Leibniz).

He proves by algebra that Hamlet's grandson is Shakespeare's grandfather and that he himself is the ghost of his own father.
(Joyce, *Ulysses*).

Then came the revelation. Marini saw the rose as Adam might have seen it in the Garden of Eden, and he understood that ... we can refer to or evoke, but never express, and that the high and splendid volumes ... were not (as his vanity had dreamt) a mirror of the world, but one more object added to the world.
(Borges).

My symbolical expression was really a mythological description of the use of a rule.
(Wittgenstein, *Philosophical Investigations*, 221.)

1.0 What is a linguistic theory to be about? Carving the world at its joints.

An initial problem for any systematic scientific study is the identification of its subject matter: there is always an indefinitely large number of facts, the difficulty lies in selecting some subset, the members of which promise to be able to be tied together by some unifying account. It is also to be noted that any selection constitutes a substantive theoretical claim about the world. In identifying a phenomenon, or general area of enquiry, we are already carving the phenomenal world at joints that are suggested by whatever categories that we use to apprehend and discriminate between the things that we notice.

For example, let's say, in a very pre-scientific way, that we are interested in creatures that live in water. We are already, in suggesting that we need a theory drawn

on such lines, making a substantive claim - a claim itself suggested by the categories of a pre-scientific apprehension - and we are also, should we attempt a theory that takes notice of this categorization, taking a wrong direction. In making this point we are merely rehearsing the consensual view of the theory-relativity of observational statements. In this case, our observation of creatures in terms of an aquatic/non-aquatic distinction is relative to a mistaken theory. It turns out that this manner of categorization, while certainly objective, carves the world at neither proper nor illuminating joints. Whales live in water, but they are to be better understood in respect of their classification as mammals: the aquatic/non-aquatic distinction is not, it turns out, an explanatorily productive categorization. Of course, we succeed in getting it eventually right, or at least less false, only by initially making such wrong moves. What is crucial is that there are means available by which the error can be recognized. This, in effect, requires that alternative ways to conceptualize are, or can be made, available. One might think of theory development as involved in the construction and deployment of such means, so re-aligning or reconfiguring categorization and providing us with, as it were, alternative spectacles by which to look at the world. As we will elaborate below, a central role in such re-alignment and initiation of new discourses is played by metaphor.[1]

For a more sophisticated example of reassessment of pre-scientific categories, consider the notion of a fish. Gould points out that membership of some species in this pre-scientific category depends on the criteria, what we have figured as "spectacles", applied to the task of determining taxonomy. For cladists in biology, for whom taxonomy is determined in respect of evolutionary branching order and shared and derived characteristics, it turns out that really "there is no such thing as a fish".[2r]

The issue is not, unfortunately, quite so clear cut, for while this conclusion "is undoubtedly true as an expression of branching order in time ... [, it might be asked whether] classifications must be based only on cladistic information." Which is to ask either of two questions: is a cladistic taxonomy in fact a case of getting it right, are there some better, more accurate spectacles? or, is there one and only one way of getting it right, must there be one and only one correct taxonomy? The assumption that there is, is the assumption that underlies the standard construal of the scientific enterprise: the sciences standardly understand themselves as making truth claims, where truth consists in getting it right in relation - a correspondence relation - to how the world is in itself.[3]

For our present purposes, however, the moral of the story is that the world does not come ready-labelled with directions as to which conceptualizations, and so

which sets of phenomena, constitute discrete and proper domains that will admit of a unitary explanation. The manner in which we categorize and conceptualize the world is no guarantee of how the world is in itself: the joints that our initial and pre-scientific conceptual scheme (or "folk theories") carve may not correspond to how the world in itself is carved, nor reflect what things there are in the world. One might figure science as an enterprise which is concerned with verifying the correspondence of some prior conceptual scheme with how the world actually is, and possibly, indeed usually, disabusing us of our pre-scientific folk-theories.[4] In our illustrative case from biology, science parcels the world into different subsets of phenomena than those of a pre-scientific apprehension. A correlative observation is that:

> Concepts that have proved useful for ordering things easily assume so great an authority over us that we forget their terrestrial origin and accept them as unalterable facts.[5r]

The point is that:

> Our intuitive assumptions, and even what seems phenomenologically obvious, may be misconceived and may thus undergo reconfiguration as new theory emerges....[6r]

These remarks may be taken as a preliminary sketch such as to remind us that there is an issue regarding how the gross phenomenon of the pre-theoretic linguistic is apprehended, and correlatively carved, by some conceptual scheme: there is no unrevisable and non-negotiable phenomenological given. The task, now, is to provide some finer detail in respect of the assumptions and conceptual apparatus that ground Chomsky's project for linguistics and to remind ourselves of their "terrestrial origin". In doing so we will be implementing a set of methodological criteria of meta-theoretic accountability - "a principle of irreduction" - that recognizes, in Sellars' phrase "the myth of the given",[7r] that there is no theoretical innocence:

> [This principle] mandates that no theoretical assumption - empirical premise, ontological framework, analytic device, investigative equipment, mathematical technique or other methodological paraphernalia - be given *a priori* pride of place. Every piece of metatheoretic apparatus should be "left open" in order

> to be subjected to critical assessment .... Unless one is willing to adopt this strict standard of suspicion, ontological biases and unwarranted metaphysical assumptions will slip through and derail subsequent analysis.[8r]

This principle suggests, for any theoretical assumption or piece of conceptual apparatus, some possible lines of enquiry. These may be expressed in terms of a commercial metaphor:

a) Where it was bought (its original home);
b) Why it was bought (the problem addressed); and
c) What is the cost, initially and in maintenance (the consequences);

These lines of enquiry - what they amount to in detail will emerge - will constitute a loose agenda for our previously canvassed "Anglo-Saxon deconstruction" of the Chomskyan paradigm.

2.0 Instituting an object of study: the formal system metaphor.

The initial problem for a scientific linguistics is to isolate and identify out of what Saussure calls "un amas confus de choses hétéroclites", which constitutes the everyday, common-sense and vague notion of "language", those aspects which are susceptible to a systematic and explanatory account.[9r] A similar observation of the scientific uselessness of the everyday conception of the linguistic is made by Chomsky: "it involves too many disparate and obscure concerns and interests. This is why [it] is useless for actual enquiry", "'language' is no well-defined concept of linguistic science".[10r] In respect of this it is needful "to determine the nature of the object [linguistics is] studying, ... without this elementary operation a science cannot develop an appropriate method."[11r]

For both Saussure and Chomsky the solution is to postulate an abstract system of underlying formal entities and the rules that mediate and relate those entities. The system that is posited, Saussure's *la langue*, Chomsky's competence grammar/I-language, is to be rigorously distinguished from all that is external to it, *la parole*, performance/E-language:

> In separating *langue* from *parole* we are separating what is social from what is individual and what is essential from what is ancillary or accidental.[12r]

Interestingly, while the Saussurean dichotomy instituted a productive enquiry into phonological systems, as instanced in the European structuralist tradition, a correspondingly productive syntactic enquiry was not also forthcoming. Chomsky suggests that this was because:

> [Saussure] regards *langue* as basically a store of signs with their grammatical properties .... He was thus quite unable to come to grips with the recursive processes underlying sentence formation, and he appears to regard sentence formation as a matter of *parole* rather than *langue*, of free and voluntary creation rather than systematic rule. There is no place in his scheme for "rule-governed creativity" of the kind involved in the ordinary everyday use of language.[13r]

Arguably, what Saussure lacked was the conceptual means to realize and articulate the idea of rule-governed creativity, i.e. the perceived systematicity of that creativity. Indeed, this creativity (Humboldt's "infinite use of finite means") is cited by Chomsky, in his early writings, as "the central fact to which any significant linguistic theory must address itself."[14r] It was arguably this lack that constituted an obstacle to the institution and development of a syntax based on Saussurean foundations. It was the development of such a conceptual apparatus in the field of mathematical logic, specifically Post's theory of recursive functions,[15r] that was enabling of a systematic and explanatory syntactic theory. (This was, in terms of our commercial metaphor, where the apparatus was purchased.) What was made available was a means, apparently, to capture just this systematic creativity, this putative central fact.[16] The privileging of this phenomenon, revised (see footnote 16) as the means to creativity, is what selected for the formal conceptual apparatus. However, for those of suspicious mind, the obverse case might be made: it was not the phenomenon that selected an appropriate apparatus, rather the apparatus selected for the central *explanandum* and this has then illicitly been construed as an unnegotiable given:

> [Chomsky] picked out for inclusion within his explanatory theory just those aspects of language which lent themselves to expression within the terms of a formal theory.[17]

The point is anticipated by Saussure, and is a corollary of his observation of the heterogeneous muddle which, seemingly, is resistant to providing any central and privileged facts for explanation, anything that is phenomenologically salient:

> Far from it being the case that the object precedes the point of view, rather one would say that it is the point of view that creates the object.[18r]

In brief, the Chomskyan paradigm and its privileged object of study - syntactic competence - was instituted on the basis of, in Lakoff's phrase, a "formal system metaphor for grammar". It is this "point of view" - not only an apparatus but also an attitude to that apparatus - that "creates the object" with a correlative "commitment to try to understand natural language in terms of such [formal] systems".[19r] It is this commitment with its associated carving of the gross phenomenon, the everyday muddle, which, in terms of our commercial metaphor, constitutes the cost of the project. What we shall be concerned with is detailing this cost and in asking whether it is adequately redeemed; whether that created object answers to some real object in the real world.

2.1 Formal systems.

Before taking up these issues, it will be useful to sketch out the notions of a formal system and of a model for a formal system, the notions that are central to "deductively formulated theory", the manner of theory institution and development in the generativist paradigm.[20]

The concept of a formal system derives from Hilbert's formalist project for mathematics. This project was intended as a response to a perceived crisis in the foundations of the subject induced by a recognition of the problematic relation between mathematics and reality, i.e. between mathematics and what mathematics had been confidently assumed, since the time of Pythagoras, to be about; i.e. the real world. The source of the problem was the development of non-Euclidean geometries in the nineteenth century.

To illustrate:[21r] the non-Euclidean systems put forward independently, but more or less contemporaneously, by Gauss, Lobachevsky and Bolyai all shared the standard axioms of Euclidean geometry with the exception of the fifth, the parallels axiom. Proceeding on the assumption of the falsity of this axiom, each arrived at a

different and consistent system. The puzzle that resulted comes from asking the question as to which system is true, or, equivalently, asking what, if anything, each system is about. Euclidean geometry had been assumed to be about (the real forms of) the real world, understood intuitively in terms of lines and planes. However, from the perspective of these new systems - sharing, with the aforementioned exception, the same axioms and so, presumably in respect of these axioms, the same content, (i.e. these systems should also be about lines and planes) - these contents of the shared axioms have to be reinterpreted: in the case of spherical geometry "line" becomes "large circle" and "plane" becomes "sphere's surface". What begins to appear is a separation of formal, proof-theoretic properties, from intuitive content; the formal properties of the axioms seem to be independent of any particular content ascribed to them: the same axioms appear to be about different and mutually exclusive things; their formal properties underdetermine their content. But then, it might be asked, what is geometry about, what is its contact with the world and with which world, for there seems to be a plurality of worlds which it can be construed to be about? But is there not only one world? The net result was, to quote the title of Kline's book, a "loss of certainty" and a loss of confidence in mathematical reasoning as providing a window on the world, as providing truth. Another result was subsequent attempts to resecure the real world foundations of mathematics elsewhere than in geometry.

The direction taken, by Dedekind and Weierstrass, was to attempt to posit arithmetic as providing that alternative foundation. The point of Frege's, and Russell and Whitehead's, logicist project was, in turn, to supply an indubitable foundation for arithmetic by deriving it from the logical operations of set theory: the notion of a set supplies the intuitive content, the operations of set theory are identified with logical relations[22] and logic is supposed to be indubitable. What, famously, stymied this move was Russell's paradox (amongst others). As Quine has remarked in a different connection, paradoxes "pack a punch". In this case a punch that was catastrophic to the whole logicist project, for what the paradoxes show is that there is a fundamental contradiction in the notion of a set upon which notion the new and secure foundations were to be raised. The result that fell out from the discovery of non-Euclidean geometry was replicated:

> The existence of these and other paradoxes served to widen the gulf between mathematics and reality .... Now one saw that statements could be made in the language of mathematics which could not correspond to any situation in the real world.[23r]

Hilbert's solution to such "intolerable" problems, i.e. problems deriving from the content/semantics of foundational notions, was to release mathematics from the burden of having any content at all: mathematics is not true of anything, but is to be considered as concerned with the study of meaningless symbols.[24r] It is a view that was rehearsed by Wittgenstein: "All mathematical propositions mean the same thing, namely nothing."[25r] In the light of this, the task of the mathematician is to:

> lay down symbols and at the same time prescribe the rules according to which they must be combined; these rules suffice to characterize these symbols and to give them a mathematical value. [The mathematician] creates mathematical entities by means of arbitrary conventions, in the same way that the several chessmen are defined by the conventions which govern their moves and relations between them.[26r]

The outcome is a concept of mathematics and, when this approach is extended to logic, a concept of formal systems in general, that are content neutral. To put this another way, we get an autonomy of syntax (the set of symbols and their combination into formulae under systematic and prescribed rule) from semantics, i.e. an autonomy of syntax from (sets of) objects of the real world that might supply the interpretations of the syntactic terms. What also appears, if such systems are to have any application through reference to real world phenomena, is the need for a means to specify content. What we get is the notion of a model, i.e. some structured set of entities, the members of which set act as the referents/interpretation of the syntactic terms of a formal system by way of a function mapping from a calculus' terms onto the individual entities of the model (and sets of entities and sets of sets of entities).

It is this idea which is at the heart of deductively formulated theory: predictions about the behaviour of real world phenomena are articulated through some calculus, through a set of purely formal (i.e. syntactic) operations together with a function mapping symbols to entities. If the formal system is up to the job of explicating the real world phenomena under study, (i.e. the real world, some subset of it, is taken as supplying the model for the formal system), then the behaviour of, and relations between, the entities in the model as supplied by these real world entities should keep step with the purely formal operations. This in the sense that, given an interpreting function, the formal operations express/predict the nature of the relations between the entities of the model. All the work is done in the formalism/syntax and it

is through the formalism/syntax that some phenomenon gets explicated, that some understanding is articulated.

In linguistics the notion of a model and of a mapping between formal terms and entities works on two axes: a syntactic - where the formalism's terms are mapped onto objects that have syntactic content - and a semantic - where those pairings of the formalism's terms with natural language syntactic objects are themselves mapped onto meanings. The application of a formal system to natural language (i.e. having natural language as the model that interprets a formal system), involves a claim about the nature of natural language: that it can be understood as itself constituted on the same pattern as a formal language, and hence also consists of a syntax that is discrete of a semantics.

A formal system is initially neutral as to its subject matter/applications, i.e. as to what it gets to have as a model. Initially it is no more than a syntax. It is not *about* syntax (e.g. of natural language); it is not, indeed, about anything, it is just a system of symbols. For a calculus to be interpreted as being about natural language there has to be a function mapping the terms of the calculus onto some model, some set of entities. In the case of a generativist grammar the calculus' terms are interpreted by sets of lexical and constituent items; for example, a symbol, let's say N, is mapped onto the set {cat, dog, house, ...}, which set is itself no more than a set of symbols.[27r] Distinct of this mapping and, as it were orthogonal to it, is the function that provides semantic content/meaning to the symbols (where "symbols" is usefully ambiguous between calculus and natural language symbols). This is the task taken up by Montague's model-theoretic project for semantic theory.

2.2 Theory constitutive metaphors, realism, instrumentalism and formal systems.

The importance of metaphor and the nature of its role in the development of scientific theories has come to be stressed in recent years:

> There exists an important class of metaphors which play an important role in the development and articulation of theories in science.... They are used to introduce theoretical terminology where none previously existed.[28]

Such "theory constitutive metaphors" accommodate our available conceptual apparatus "to the causal structure of the world", they provide a certain epistemic access and so institute and enable a domain of discourse by hypothesizing that one can understand the previously ill-understood by transposing onto it, and thereby articulating it, in terms of the properties of the well-understood. To borrow again Wittgenstein's image, such metaphors are like pairs of spectacles which enable us to ascribe, or at least hypothesize, properties to whatever we are looking at. Fundamental to these remarks is the observation that hypothesis formation is limited by and limited to the set of concepts (spectacles) that are ready to hand: "it is undeniable that the patterns we can make explicit are limited by the material available to our imaginations."[29r]

A useful informal example is the metaphor of mind as computer. We understand computers and we do not understand minds. By hypothesizing the mind *as* a computer we institute a research program which is designed to test the extent to which the metaphoricity can be removed, to test whether, indeed, the mind can be understood in computational terms. Note that, in the same movement, one also implicitly carves the phenomenon of mind at some joint, i.e. precisely at the parameters that are defined by the computational model, for example one invokes a distinction between program and algorithm. One can note in the literature a tendency (strong AI) to excise the metaphoricity, to take the hypothesis as confirmed, and to more or less assume that the mental is coterminous with the computational: if some phenomenon cannot be explicated in computational terms then its not mental. The sceptical rejoinder[30r] is standardly to point out that this leaves out some crucial element (usually *qualia*, the "feel" of mental experience) which is evidence either that the computational metaphor is simply mistaken, or that it has to be supplemented. Either way, the computational metaphor carves the phenomenon of mind at joints that are at least questionably in correspondence with the nature of the actual phenomenon. On the positive side, adherents can point to the productivity of the primary metaphor, its "heuristic fertility" in instituting a domain of discourse whereof before we had to remain silent.

These remarks raise a central issue in respect of the institution of a domain of discourse by way of some theory constitutive metaphor: whether the initial, heuristic metaphoricity is eliminable, and whether there is sufficient warrant for its elimination. The issue is that of (dis-)confirmation of hypotheses, where those hypotheses are taken to be making a realist truth claim. The elimination of the metaphoricity marks

the movement from a discourse regarded as an instrument, as a means to talk about and characterize some phenomenon, to that discourse being regarded as providing an account that is true because corresponding to facts of the matter, which facts are inherent to the object of enquiry.[31] It is the movement, in short, from hocus pocus to God's truth, to a theory being taken as true because that theory corresponds to how the world in fact is. Grounds that may be, and frequently are, adduced for this movement being precisely the "heuristic fertility" alluded to above.[32] The claim here is that a study would not achieve success, we would not be able to say so much, so apparently accurately, on the basis of merely a metaphor. (But then again, to get ahead of ourselves a little, the same claim to heuristic fertility could be made for our category of fish, a category to which, it can be argued, nothing, in fact, answers.)

To use some conceptual apparatus as a metaphor and means to do no more than institute talk of some object is harmless, it is no more than to characterize. Typically, the sciences understand themselves (and the Chomskyan paradigm understands itself - "a theory carries a truth claim if it is serious"[33r]) as attempting more than characterization, more than talk about some object, rather they attempt some expression of properties that are inherent to the object under study. The realist assumption is that there are determinate and determinable, mind-independent facts of the matter, the discovery of which is what counts as getting it right. In the case of linguistics, it is just this move from a construal of the enterprise as a characterizing enterprise to one which aims for, and supposes the propriety of, a realist interpretation that is the second in our list of the principle strands of Chomsky's revolution. It is the achievement of adequate warrant for such a realist construal (the redemption of the cost of the initial metaphor) with which we will be much concerned.

The distinction between these two construals of the linguistic enterprise is reflected in the distinct perspectives which realism (the "God's truth" view) and instrumentalism ("hocus-pocus") have in regard of theory-articulating calculi. Realism involves the belief that:

> the world [is] a totality of language[/calculus]-independent things, a totality which is fixed once and for all, and, ... , one (and only one) reference relation connecting our words[/notation] with that totality is supposed to be singled out by the very way we understand our language[/calculus].[34r]

Consequently, in respect of a calculus, realism requires, in the linguistic case, that:

> the constants abstracted in an analysis, if the analysis is correct, are in some way inherent in the actual material of the language under analysis. [When the realism is given a psychological ontology, then these constants] are part of the content of the speakers' minds or brains.[35r]

On the other hand, an instrumentalism towards a theory (which need not be accompanied by a corresponding anti-realist metaphysics) involves no such correspondence between the terms of a calculus and any putative real entities:

> the linguist's abstractions have no other status than as part of his scientific terminology, and are justified by their utility in stating regularities and making predictions about the forms of utterances ...[36r]

In linguistics, as we have canvassed, a formal system provides a conceptual apparatus that, in, as it were, seeking a model, makes possible a domain of theoretic discourse by way of the terms and relations of some calculus being hypothesized to refer to entities, and relations between those entities, in some real world phenomenal domain. This hypothesized domain, as articulated by way of a formal system (together with an interpreting function), carries with it both external and internal phenomenological boundaries. In the former case, this is by carving a discrete phenomenological domain from the manifold phenomenological world (e.g. we are deciding to talk about language, not physics). In the latter case, we are making internal distinctions and positing constructs that interpret the terms of the calculus within the selected domain. Such hypothesized "joints", the costs of deploying formal systems, need to be redeemed, which is to say, they cannot be assumed, without some further argument, as givens, but must be available to question.

For example, simply because we have well-defined notions of syntax and semantics provided by the study of formal systems does not mean that, necessarily, nor even that we should expect, some real world object to answer to that pattern. Such would be to assume that, because we have a concept or some conceptual apparatus, then there must be something in the world to which it refers and by virtue of which the concept has meaning (consider phlogiston or unicorns, for example, or, more pertinently, the concept of a fish or some areas of mathematics that have no known application).[37] To make such an assumption would be to assume that the world is prefigured by, and designed on, the model of our conceptual scheme. To borrow a

phrase of Chomsky's, it would be to institute an enquiry into a system of the real world on the assumption that the world conforms to some sort of "virtual conceptual necessity",[38] as if the world is guaranteed to provide models for axiomatic systems, as if we already see such models there independently of theory. Certainly, such a demarcation carves language at some joint, but the joint is that of the conceptual scheme we apply to the task of talking about the object of study. The question is whether it corresponds to the joints the world carves in itself, and so to the properties of some real world object. Alternatively, and what might bring to question our use of the term "metaphor" in respect of formal systems, is whether any credence and content can be given to this notion of conceptual necessity.

Deferring this latter question for the moment, and anticipating the answer to be in the negative, the conclusion we are drawn to is a rehearsal and reinforcement of the observation of the lack of unrevisable, pre-theoretical givens. In acknowledging this, we may also notice what is, perhaps, most remarkable; that is the extent to which the substantive truth-claim that is brought in by way of Chomsky's foundational metaphor (i.e. that natural language is designed on the model of a formal language) has come to appear so natural, so seemingly indefeasible - at least to (many) linguists - somewhat in the manner of a linguistic counterpart to the commonsense category of fish: in this latter case, it seems we just cannot help but see things, despite what biology might tell us, in terms of that categorization. In linguistics the result of a similar reflex is, arguably, that the metaphoricity is forgotten and the truth claim that is made on the back of the formal system metaphor takes on the appearance of an indefeasible obviousness. To reprise Wittgenstein's remark, it is rather as if we forget that we are wearing certain conceptual spectacles and, at the same time, it is to reinstitute some given, forgetting that any given is likely to be no more than a myth.

The point is made by Putnam. It is simply not the case that the models that are the interpretation of formal systems, the referents of theoretical terms, are simply stumbled across in the world, as if we do, indeed, already see them there, because they are there, prior to theory. Rather the entities and phenomenological parameters that supply the content of formal systems are projected/hypothesized onto the world by our theories; the models themselves are products of the human mind, there is no direct access onto the world in itself:

> Models are not lost noumenal waifs looking for someone to name them; they are constructions within our theory itself, and they have names from birth.[39r]

When understood, initially at least, as metaphors, the constructs and demarcations brought in by way of a discourse articulated through formal systems amount to no more than, and no less than, hypotheses. As such there is nothing to which exception may be taken, at least there can be no objection so long as such hypotheses remain available for disconfirmation and are not simply assumed. The question, and so the possible grounds for disquiet, is whether a postulated object/model, a competence grammar, can be sufficiently discerned (the condition on it being possibly disconfirmed) as some real, independently existing, theory-external object. Such would fail to be the case (and we will argue that it does fail to be the case) if the data were, in practice, insufficient to, or precluded from, the task of disconfirmation; for example if the data, and any possible data, were interpretable by the theory so as always to fit the theory. The result would be that rather than the investigation properly asking what is required of the formal apparatus if it is to correspond to the facts, instead the converse would apply and the object of study would be required, and would be interpreted, to conform to, i.e. satisfy, the parameters and terms of a calculus. Any apparent success of the theory would be achieved by way of answering to an object of study which is itself an artefact of the theory and of the conceptual apparatus through which the theory is articulated: the object of study would indeed, to reprise Saussure, be created by the linguist, but would be no more than the creation of the linguist, because not answering to, and this because, as it will turn out, it is insulated from answering to, any real world object. In short, it would be no more than a fiction.

The point is quite general:

> Suppose an account unquestioningly relies on some category α - by blindly imposing it, deferring to it, reducing other phenomena to it, or in any other way using it without explanation. It is not too much of a stretch to realize that the ontology [and properties] of the target subject matter is liable as a result to be biased in α's favour.... Thus if one is committed to the use of mathematical methods, chances are that one will find the phenomena in one's area of inquiry to be of the sort to which mathematical methods apply. If one is pretheoretically committed to formal methods, certain kinds of ambiguity are less likely to be accorded theoretical centrality. ...
>
> What is at issue, especially when the ontological status of the subject matter is fragile, is any a priori or

> advance commitment to such categories. ... To give prior allegiance to any such categories or techniques is prejudice, in the sense of being "pre-judged."[40]

To investigate these issues, we need, firstly, to substantiate our claim as to the initial metaphoricity of any formal system, and, secondly, to examine more closely the role of formal calculi in the development of linguistic theories. This will amount to an enquiry into the manner that the forgetting of the metaphoricity, and so "the myth of the given", takes in Chomskyan linguistic theory. In chapter II we take up the issue of conditions to be met such as to provide some confidence in the possible excision of the metaphoricity and so the possibility of a realist construal of a scientific theory.

3.0 Theorizing Galilean style: formal systems and metaphysical realism.

The use of formal calculi in linguistic theorizing is paralleled by the reliance on the modelling ability of mathematics in much of the natural sciences. This style of theory construction Chomsky refers to as Galilean.[41r] Underlying the emphasis on the mathematization of science is, in some cases, an almost mystical belief in the ability of mathematics to articulate the real, inherent-to-the-object nature of things, a sort of latter-day "all things are of number" Pythagoreanism:

> Philosophy is written in this grand book, the universe, which stands open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics.... (Galileo[42])

While the idiom of the above might be thought somewhat quaint, it is a position that still has adherents, unsurprisingly, perhaps, more frequently amongst mathematicians:

> Einstein was not just noticing "patterns" in the behaviour of physical objects. He was uncovering a profound mathematical substructure that was already hidden in the very workings of the world.[43r]

(As we will note, this is not a universally accepted interpretation of the status of mathematics, Einstein, for one, appears to dissent.)

The remarkable success of this Galilean approach to science raises a deep question as to why, apparently, mathematics allows such access into the nature of things; what is it that gives rise to "the unreasonable effectiveness of mathematics in the natural sciences"?[44]

One answer that might be suggested goes as follows: somewhat in keeping with the above quotations, God (either literally or in the guise of creation) is a mathematician and mathematical forms, the object of His contemplation, prefigure the structure of the world. That world is determinate, containing a totality of objects of which the properties are mind-independent; furthermore, that world's structures and objects are (perhaps not exhaustively) determinable by us through the identity of our language, in this case mathematics, with the language of the world's design which is, as it were, written in the world. Mathematics is a transparent language and gives directly onto the world. Truth, on this scheme, is a relation of correspondence between language and things, and the corresponding theory of meaning is unreservedly referential (compare footnote 37). To give this view a label we may call it (mathematical) metaphysical realism. It is this view, we will suggest, that underlies, in the sense of supplies the metaphysics of, the Chomskyan paradigm. Interpretably, it is this implicit metaphysics that gives rise to what we aim to show amounts to the Chomskyan paradigm's unreasonable reliance on formal calculi in linguistics; a reliance which amounts to a forgetting of their metaphoricity by way of an illicit conceptual necessity, as if the world has to conform to some available idea of it as articulated by a formal calculus.

The above answer to the question of the effectiveness of mathematics raises a further question as to how we sublunary beings get to be in any sort of knowledge relation with these mathematical forms that prefigure the world and which the world instantiates. (As Einstein remarked, and Chomsky has echoed (1980, pgs. 7 ff), what is deeply mysterious about the world is its very understandability.) Plato supplies a somewhat mystical answer: these forms are mirrored in our "pristine nature", "the motions akin to the divine part in us are the thoughts and revolutions of the universe."[45] It is an answer worth mentioning because it gets recapitulated by Chomsky, only the mysticism is dressed up in a biological guise: the success of the Galilean style in the natural sciences might result "from chance convergence of biological properties of the human mind with some aspect of the real world."[46] It seems we need to invoke such "clouds of biological glory" (Ryle's phrase) to account for the fit between our conceptual apparatus and the world which we figure through

that apparatus. (The same role as Ryle's clouds have (i.e. as *explanans* and so guarantor of a correspondence between mind/language and reality, as guarantor of some form of conceptual necessity), is, in idealist metaphysics, played by God in His Greek guise as Λογοσ - see, for example, Leibniz' *Monadology*. God is also, of course, what allows Descartes to dispel his epistemological doubts.) However, this need for divinity, mysticism or chance in account of this fit, indeed, the very cogency of asking the question about this fit, is sufficient to provoke the possibility of the suspicion, that it is, in fact, illusory; that our conceptual apparatus is not an undistorting mirror of the world, but rather, in line with Putnam's contention (and Borges' distinctly post-modern revelation (see the prefatory quotation)), it is one more object added to the world and that "our way of understanding the world ... is an imposition of our conceptual schemes upon external reality".[47] This suggests an alternative and more sceptical response to the question raised by Wigner's observation of the unreasonable effectiveness of mathematics: that the fit between mathematics/formal systems and the world is, or can be granted to be, no more than apparent: it does not reveal the inherent-to-the-world properties, but rather provides us only with, in Wittgenstein's phrase, a "mythological description"; that is a world interpreted in terms of our interests, in terms of our means to gain epistemic access, and being the only means it is mistaken, when conjoined with the natural impulse to a metaphysical realism, for the world in itself.[48r]

3.1 The problem for (mathematical) metaphysical realism: why a calculus as metaphor; why there is no conceptual necessity.

It is often implied that the correlation of mathematical systems with the physical world is absolute and exact, hence the apparent "*miracle* of the appropriateness of the language of mathematics to the formulation of the laws of physics".[49r] It is this observation which grounds the Galilean/Penrose view, expressed above, that the world is (with a minimum of metaphoricity) mathematics, that the mathematical representation of the world (i.e. the models that interpret mathematical systems) can be more or less conflated with real-world, mind-independent phenomena. And it can be so conflated, goes the reasoning, because the world is inherently informed (in the sense of "given form") by mathematics; nature is intrinsically mathematical: a mathematical system uncovers, because it is identical with, Penrose's profound substructure. Interpretably, a similarly metaphysical realist

view is evident in Wittgenstein's *Tractatus*: a mathematical "picture" of the world is correct when the logical structure of the mathematical language/notation corresponds to the structure of what is depicted:

> There must be something identical in a picture, and what it depicts, to enable the one to be a picture of the other at all.[50r]

"The logico-syntactical forms [of the notation] mirror the metaphysical forms of the objects they stand for."[51r] It is a simple step from this to taking it that the reason why a mathematical calculus achieves "coverage" is because that calculus correctly mirrors these metaphysical forms, which forms are inherent in the object represented. What is enabling of the "coverage" is the sharing of the same "metaphysical forms". In such a manner the unreasonable effectiveness of mathematics is "explained" by the language of mathematics being construed, (forgive the grandiloquence,) as the language of Creation.

There is, as advertised, some quite authoritative (Einstein's) dissension from this view:

> So far as the laws of mathematics refer to reality, they are not certain. And so far as they are certain they do not refer to reality.[52]

What undermines confidence in the identity of the truths of mathematics and of nature, and so what damages the transparency of the relation between mathematics and the natural world, and damages the view that the world conforms to some mathematical (or formal system) conceptual necessity, is the plurality of axiomatic systems in account of the properties of the same set of objects or phenomena and, conversely, the plurality of sets of entities that satisfy (are potential models of) any one axiomatic system.[53] Indeed, it is this very plurality that requires a reinterpretation of mathematical calculi as non-transparent, as related to the world only through some (or several) interpretative function mapping terms onto entities. (Note that these remarks are a formal science counterpart to our previous discussion of the informal category of fish.)

To reprise, with a slightly different modulation, our previous discussion: what brought to question the apparently direct epistemic access onto the world provided by axiomatic systems was the development of non-Euclidean geometries. These several

different geometries fit spatial experience equally well. If it is claimed for one of these geometries that it reflects some intrinsic mathematical substructure of reality, then cannot this also be claimed for the other geometries? But then, there can be only one such real substructure: not all, indeed not more than one, can be true in the sense of corresponding to some mathematical substructure inherent in world objects. But if all are indistinguishable in respect of truth, then this infects the idea of the metaphysical reality of mathematics, that some mathematical system is properly true, intrinsically of some aspect of the world.

This plurality of geometries was duplicated in the twenties in the field of logic with the demonstration (by Lukasiewicz, Post and Tarski) that there could exist any number of consistent logics based on different axioms. The upshot, according to Church, is that:

> We do not [and can not] attach any character of uniqueness or absolute truth to any particular system of logic ... We may draw the analogy of a three dimensional geometry used in describing physical space ... there may be, and actually are, more than one geometry whose use is feasible in describing physical space. Similarly, there exists, undoubtedly more than one formal system whose use as a logic is feasible, ... it cannot be said that one is right and the other is wrong.[54r]

The conclusion that is arrived at is that:

> apparently mathematical design [is] not inherent in nature, or if it [is], man's mathematics [is] not necessarily the account of that design. The key to reality has been lost.[55]

Given this plurality, or, to illuminatingly over-extend a phrase, given such extensional equivalents, (an illustrative linguistic parallel with the geometric case in mathematics being the equivalence of pure categorial and context-free grammars,[56]) then any choice between them is to be made, not in respect of their truth, but in respect of the applications to which they are each best suited. Either this or "some additional correspondence principle must be used to judge whether a piece of mathematics applies to the real world".[57r] In short, "coverage" is not by itself adequate

evidence of the truth of a formal/mathematical system in respect of the properties of a phenomenon modelled by that system.

The observation of the lack of inherent fit and contact (one might think of it also as lack of transparency) between mathematical/formal systems and real world phenomena results, as we noted, in the reconstrual of formal systems with their attendant models as involved in a metaphorical relationship with objects and phenomena in the world. The result of this reconstrual is a contentful and proper distinction between the vehicle for articulating theoretical claims, in this case the mathematics, and that which is modelled, the objects of the world. Moreover, beyond recognizing this proper distinction, the observation also remarks (compare Einstein's comment above) on the possibility of an ineliminable discrepancy between the modelling ability of mathematics and the things that it is used to model; that the mathematics is not some transparent medium, or undistorting mirror, but one more object added to the world, through the agency of which we gain some means to talk about that world and by which we carve/conceive the world such that it supplies a model that satisfies the terms and relations of some calculus. That there is such a possible and discernible discrepancy characterizes the mathematics, on the one hand, as a vehicle that is instrumental (i.e. acts as a metaphor) in articulating the nature of phenomena, and, on the other hand, characterizes the properties and parameters of the mathematical system as logically and ontologically distinct from, and autonomous of, those of the real world object. It is a distinction that is required to be observed:

> For we can avoid ineptness or emptiness in our assertions only by presenting the model as what it is, as an object of comparison - as, so to speak, a measuring-rod; not as a preconceived idea to which reality *must* correspond. (The dogmatism into which we fall so easily in doing philosophy.)[58]

In the following our aim will be to trace such a "dogmatism" (effectively, what is, in Smith's phrase, "pre-judged") and its consequential costs in the development of theory within the Chomskyan paradigm for linguistics.

4.0 Linguistics Galilean style.

The extension to linguistics of a Galilean (mathematical) metaphysical realist attitude to formal/mathematical systems deserves a name. We might term it Fregean (or, in view of our prefatory quotation, Leibnizian, or even, see below, Quinean[59]) and intend by it, firstly, the observation that the formal properties of a calculus drive theory construction, this being the core of deductively formulated theory and post-structuralist "top-down" theorizing,[60] and, secondly, the tendency to take certain properties and parameters of the modelling calculus as more real than, or at least as the guarantors of, the reality of that which is being modelled, as if the interpretation of the formal system has to be found in reality. It is as if the mark of the reality of the object of study is its ability to answer to the terms and parameters of a formal calculus: it is to have a quasi-metaphysical expectation that the system under investigation is some instantiation of a prefiguring calculus, that it answers to such conceptual necessity; in other words (Wittgenstein's), it is to take a calculus as a preconceived idea to which reality must correspond. But, who is to say that the mark of some object of study's reality is its ability to be captured in a formal theory, or, indeed, that a mathematical or logical apparatus is, necessarily, as if on some sort of *a priori* grounds, the appropriate language in which to describe, carve and capture a phenomenon's intrinsic nature?

That such a view is entertained, at least implicitly, is part of linguistics' inheritance from Bloomfield's and the fifties' positivism. (Recall it was Bloomfield who suggested the desirability of the mathematization of the subject.) For the Logical Positivists (of whom we take, following Quine,[61r] Carnap as the "embodiment") the approved model for a scientific theory was provided by Mach's arrangement of Newtonian mechanics into a deductive system: Newton's laws give the axioms, and the interpretation rules map the variables onto measurements of mass and time. From this basis everything else gets deduced (one might say "generated").

> The Vienna Circle ordered all science to conform to [this] model. ... To do science, you should:
>
> 1) Choose basic observables. [In generativist linguistics, sentences.]
> 2) Find formulas for their relationships.
> 3) Express all other observables as functions of the basic observables.

> 4) From (1, 2, 3) derive the rest of the subject by mathematics.[62r]

The presiding idea, as Carnap puts it, is that "the logic of science is nothing other than the logical syntax of the language of science."[63r] What emerges is a (Galilean) idea of formal systems as constituting some sort of logical and real cement of the universe. This view gets its most forthright expression in Quine's famous contention that "to be is to be the value of a variable".[64r] It would seem that the criterion of existence is the ability to be prefigured in some formal system or, as Hersh puts it, it is "like a monomaniac photographer saying, 'To be is to be recorded on my film,' or Geraldo Rivera saying, 'To be is to be seen on the Geraldo Rivera show.'"[65r] A formal system does for Quine what God does for Leibniz (and Berkeley), it is some sort of guarantor of reality: *esse est percipi* through the agency of a formal system.

Where this tendency pertains the upshot is an enquiry that is not so much about the nature of natural language as modelled by some calculus, rather the study would become one of formal calculi *simpliciter*; the supposed object of enquiry would be, to put it somewhat poetically, no more than the shadow cast by the calculus. In a less virulent form, a symptom of such a tendency might be identified, in Higginbotham's words, as "an over-emphasis on the power of notations to effect explanations."[66] The reason why the calculus keeps step with the phenomena is because the properties of the calculus correctly mirror the properties of the object under study.

This is the claim. However, there are two general grounds to demur from any realist construal of such a (non-dis)-confirmed theory. Firstly, as in the case of mathematical/geometrical models of the physical world, the case of two or more extensionally equivalent grammars where the models are non-identical provides grounds to at least defer such a realist construal. Secondly, while the formal system metaphor drives theory construction, what it also drives is a series of sub-hypotheses - in effect a modularization of the gross phenomenon - which enable data to be interpreted in such a way as to preserve and make unfalsifiable the initial hypothesis. The result is the unavailability, in practice, of any potential point of view which is not consistent with the literality of the formal system metaphor, as if there could be no other perspective. This is to assume as given what is properly requiring to be confirmed; it is to assume a certain conceptual necessity.

The task in the following is to substantiate and illustrate this charge. In doing so we will also generate and, to coin a phrase, pre-hearse a series of issues that will constitute an agenda for later discussion.

4.1 Top-down theory construction, realism and psychologism.

Theory construction in the Chomskyan paradigm is driven by the properties and parameters of the formal systems that are set to the task of modelling linguistic phenomena; it is these systems which provide epistemic access and institute a domain of discourse.

Standardly, some framework is instituted and argued for on the basis of its ability to neatly handle a certain restricted set of data, some fragment of a language. This constraint to working on fragments is a function of, to use Postal's and Langendoen's phrase, the vastness of natural languages: one cannot stand back far enough to see the object whole. Given this starting point, the subsequent analyses invoke the formal properties of the calculus applied. These analyses and a set of constructs, i.e. those entities and relations that interpret the calculus, are then carried over, as an assumption, to the task of broadening the framework's coverage: given the success of X theory in respect of a sentence type A, then assume theory X in respect of the analysis of sentence type B. The economy of information, the set of constructs and relations, that is instituted by the initial analysis is then carried down to (or recycled in) successive analyses as an assumption.[67] This in turn drives the next analysis by directing hypotheses, and this the next, with each successive move, assuming it is successful, feeding back to reinforce the preceding ones.

The observation to be made is that it is the nature of the initial hypothesis, and the properties of the formal apparatus that articulate that hypothesis, that drive and create subsequent analyses with their attendant constructs: in brief, "notation systems [i.e. calculi] create analyses"[68r] and, correlatively, different notations, i.e. different formal systems, create different analyses, requiring different sets of entities to act as the models that interpret the terms of the notation system. One of the factors at play here, and what affects the initial choice of the system, is the originary fragment that is addressed. For example, if the core problem is perceived to be long-distance dependency, then one selects a system of more than context-free power. On the other hand, if one considers that "constructions that have been shown to require greater power than context-freeness are somewhat unusual, and apparently not run of the mill

grammatical phenomena,"[69r] then one selects, perhaps, a phrase structure grammar with its formal properties and with a corresponding formal constraint on possible analyses.

Let's accept that a significant fragment of natural language happens to be characterizable by a grammar, we can say that it offers a predictively accurate account of acceptability judgements of speaker-hearers of the language.

We may be content with what we have and view our grammar as an efficient and accurate means to represent, that is characterize, the syntactic facts of the matter. What is lacking, though, is an account of why the facts of the matter happen to be as they are. So far our grammar has no more than an instrumental status: it is an instrument that gives us a means to characterize the language, at least characterize it on the syntactic axis.

One might, subsequently, be tempted to go one step further (this is the move from "hocus pocus to "God's truth") and claim that the reason why the grammar is able to characterize the language is because the carving of the phenomenon at these joints and with these terms and relations corresponds to joints and to objects and relations that inhere in the object under investigation. The real properties and parameters of the formal system correspond directly to real properties of, and to real joints in, linguistic phenomena, not just under description by the grammar, but these properties are in and of the language itself: there is, for example, an autonomous syntactic component; transformations, or whatever relations and entities are posited, exist not just as properties of the mathematical apparatus, but independently of that apparatus as real, constitutive elements of natural language. In short, the properties of the modelling calculus are taken, when that calculus is shown to be predictively successful, to be real of that modelled phenomenon.

A first issue that offers to make problematic this manner of theory development, and which is potentially undermining of a realist construal, is the issue, as canvassed, of extensionally (Chomsky prefers the term "logically") equivalent grammars: simple ability to provide coverage, no matter how extensive, is insufficient to warrant acceptance of these constructs as real, as "uncovering a profound [and real] mathematical substructure", this for the simple reason that, to repeat the findings of Lukasiewicz, Post and Tarski :

> A grammar is but an axiomatized theory, and it is a truism that a theory that can be axiomatized at all can be axiomatized in radically different ways.[70r]

If one is to be a realist about the terms and relations of a grammar, what we might term its economy of information, then given that realism is committed to the view that there is one and only one true theory, then this multiplicity of true theories is something of an embarrassment: plurality, with no means to select amongst equivalents, casts suspicion on the reality of such entities by virtue of the deferral of the confirmation of any particular claim. This deferral not only preserves the question as to which one, but also asks the question, the Quinean question,[71] as to whether there is any truth issue at all. What is needed is some account of notational variation,[72] but for this to have point it is necessary, firstly, that it is specified what entities one is committed to, i.e. what of the formal system is claimed to correspond to elements and properties of the real world object care of some explicit interpreting function and, secondly, that the existence of these entities can be verified on the basis of appropriate evidence, and which evidence must be a supplement to the attainment of coverage. To put this another way: realism toward the terms and relations of a formal system requires that there are facts of the matter that are, in principle, ascertainable and that constitute a truth-issue in respect of the choice between grammars. The general point that we have rehearsed is made by Chomsky:

> Substantial coverage of data is not a particularly significant result; it can be attained in many ways and the result is not very informative as to the correctness of the principles employed.[73r]

What is needed is some additional correspondence principle (or principles) such as to provide confidence that what allows for decision between frameworks and their analyses is the putative real object of the real world.

A reason why such disconfirming contact is frustrated, and in such a way as to question the propriety of a realist construal, is a certain theory-internality that might be observed in our above description of theory development. Given the blunt, radically-in-need-of-interpretation, yes/no/maybe nature of speakers' intuitions, the constructs posited are only observable and identifiable in respect of the available analyses, and observable in respect of their ability to provide coverage (it is by way of the vocabulary provided by the calculus that we get to refer, it is how we apprehend the objects of enquiry in any detail beyond that given by intuitions).[74] But, coverage is, as we have noted, not sufficient in itself.

One possible riposte to these remarks, with their insinuation of unfalsifiability, might be that, if such a caricature of theory development were the case, then surely

there would never be any reason to revise any framework, nor initial hypotheses/analyses made within frameworks, and this is clearly not the case. It is to be admitted that our account is, indeed, a caricature, because considerably idealized. Also admittedly, there are, indeed, examples of revised and abandoned frameworks. However, again at a very general level, it is worth enquiring into the reasons for a framework being discarded.

One of the driving concerns within much of grammatical theory is to maintain the "purity" of the calculus and one of the primary motivations has been to keep the grammar as uncluttered and economical as possible. When the preservation of certain initial assumptions has led to an increasingly baroque superstructure the tendency has been to revise from the foundations upwards. For example, increasing numbers of transformations become unwieldy and the revision results in Government and Binding Theory in which there is a single transformation, move α, the operation of which is mediated and limited by a series of constraints. Minimalism appears to be going in a similar direction as does HPSG: a clean basic apparatus sprouts more and more features.

The general point is that "simplification achieved in one area of the grammar leads to complications in other areas."[75r] It is to be noted that it is not so much the raw data that are driving this revision. (It is not, to my knowledge, generally the case that frameworks get superseded on the basis of a proven inability to cover the data, there is always room for some manoeuvre, some added feature, or some hiving off onto performance or whatever. Some examples are provided below.) Rather, what drives the revision are theory-internal and methodological considerations of economy and elegance. If such criteria are taken to be alone pertinent to the task of selecting between grammars, in the sense of constituting an additional correspondence principle, then this is to assume that the real object of the real world under investigation is also designed with economy and elegance in mind. This is a species of "Galileanism", an assumption that things conform to some form of conceptual necessity: that the world is prefigured in some formal system, is some calculus writ large and that methodological criteria for selecting between formal systems *qua* formal systems are appropriate for selecting between theories in respect of their correspondence to the facts of the matter in the real-world object under investigation.

It seems that we are still in search of some contact with the facts of the matter. In noting this, one also notes that the condition on it being possible to fix on one grammar rather than another is that there are, indeed, determinate facts of the matter, the correspondence with which would count as getting it right. And for these facts of

the matter, or the matter of the facts, we need some ontological status. The Chomskyan hypothesis is, and here we pick up on the third strand of Chomsky's revolution, that the facts of the matter are psychological and are in respect of a mentally realized I-language: a set of "mentally represented cognitive structures".[76r] The psychological ontological status of the I-language provides a locus, something determinate and, according to Chomsky, some ultimately neurophysiological fact of the matter for a linguistic theory to be true of. It is this locus which supplies the potential content to the "principles" cited in Chomsky's recognition of the problem we have been discussing.

It is by way of the psychological claim that the prefiguring relation between a mathematical/logical apparatus and the object of study is, as it were, naturalized, it is how linguistics gets to be, according to Chomsky, one of the natural sciences. Linguistic objects, i.e. real objects of the real world, are prefigured (and so to be explained by) a formal system because, in effect, a mentally realized and represented formal system is hypothesized as being constructive, "involved in the aetiology", of linguistic objects. The calculus is the explanation.

One may not necessarily object to this psychological and causal claim as a hypothesis, but one is warranted in requiring that the hypothesis is testable and the psychologism given content. And this requires more than simply ascertaining the contents of a grammar and its economy of information, one also needs the hypothesis of a competence grammar (I-language) as a putatively *causal* explanation of the nature of the linguistic to be disconfirmable. It is not as if there must be a mentally realized competence grammar, as if this were the only logically possible (as opposed to only presently and apparently available) way to explain the nature of linguistic phenomena. This point is difficult to maintain sight of, because there is a distinct sense in which, indeed, there must be a competence grammar; there must be some way, probably several, of characterizing natural language strings in terms of some formalism, for surely everything has some/several accurate description(s). The crucial point is that such a means to an accurate description need not also directly supply the explanation: what is captured by the description, what one knows when one knows a language, is not itself the explanation, but is, rather, what is required to be explained. To simply assume that it is would be to be guilty of the fallacy *post hoc, ergo propter hoc.* Of course, it is not the case that such a conflation of description and explanation is simply assumed, rather the explanation is by way of the additional and logically distinct claim that that body of knowledge is mentally represented. The question, though, concerns what additional evidence there might be for this ratcheting up of a

characterization to a causal explanation; what evidence there is for a psychological ontological status for the linguistic, as defined in the theory as competence, and what evidence there is for the carrying over the demarcations introduced by the conceptual apparatus we apply to in characterizing the linguistic onto the economy of the mind.

As we have discussed, and perhaps laboured, while we may have perfectly coherent and objective categorizations care of mathematical logic (just as we have a perfectly coherent and objective category of fish), such need not provide the correct access to the general phenomenon, i.e. it need not answer to the putative real facts of the matter. Once we recognize this, and remind ourselves of where the conceptual apparatus was bought, that as a calculus it is initially neutral as to what it gets to be in a modelling relation with, then we also come to recognize that there is no *a priori* reason why mathematical logic should provide an appropriate language with which to explicate the nature of natural languages, nor why the modularization and the terms and properties that come with it should carry through to some causal and psychological explanation. It may, but it cannot be simply assumed.[77]

It is to be emphasized that in raising this point, we are not questioning the need for an account of phenomena that we apprehend efficiently and objectively in terms of the categories provided by a formal system. Indeed, precisely those perceived patterns and relations are to be explained. However, what is being raised is the possibility that phenomena need not be accounted for in terms of a theory that addresses the problem directly at the level of those patterns and relations as perceived, and as perceived through the representational medium of a formal system. It is possible that certain phenomena are, to some greater or lesser extent, epiphenomenal: incidental products of some set of causes, which causes make no direct reference to entities at an independent level of description provided by standard, formal models. [78]

4.2 Modularization.

To recap a little: the realist claim is that the way the grammar carves the gross phenomenon corresponds to real joints in the phenomenon itself. What you get is, firstly, the positing of a discrete object of study and nomological domain: a competence grammar. Under a psychologist construal what you also get - one of the costs of the project - is a corresponding modularization of mind: a mentally realized "language faculty" which is independent of other and general cognitive abilities.

A first demarcation is that between competence and performance which comes in by way of a disclaimer in regard to having anything to say "about how the speaker or hearer might proceed, in some practical or efficient way, to construct some derivation".[79r] This distinction is a direct corollary of the formal system metaphor: generative grammars licence a determinate set of strings and, for any string consisting of terms of the language, a grammar provides a decision as to the inclusion or exclusion of that string from the set. In other words, grammaticality, where it is defined by a standard formal system, is akin to the property of bivalence in logic: a proposition is either true or it is false, with no middle term; a string of natural language terms either is or is not a member of the set of sentences of a language. If the object of study for linguistics is going to be similarly determinate, then one needs to idealize from the standard run of recognizably "degenerate" utterances which litter much linguistic behaviour. The upshot is that we cannot base a study on corpora of utterances and, furthermore, it is this ability to recognize degeneracy that reflects our linguistic knowledge. It is this, our competence, which is taken to be the proper object of study. Along with this rationale for the positing of competence as the proper object of study comes, as a corollary, a distinction between (abstract) sentence and (physical) utterance, and so, implicitly, a type-token relation between the two.[80r]

Parallel to a competence/performance distinction (defined over syntax) is the demarcation between semantics and pragmatics. In both cases the former term constitutes the privileged and notionally prior area of enquiry. In semantics the privileged objects of enquiry are sentence meanings, defined in terms of fixed truth conditions that are independent of (and prior to) the individual use(s) of a sentence by a speaker, which latter subject is the province of pragmatics. Pragmatics is concerned with what speakers do with the originary, always and already, propositional content of sentences.[81] It is this propositional content which is the primary interest; for not only, from the linguist's point of view, are sentence meanings prior - they are what is worked on by a pragmatic component - but, from the philosophers point of view, they are where is achieved the originary and transparent connection between language and world.[82]

In general, what is preserved by this approach, indeed, what it is premised upon, is "the linguistic relevance of the entire body of logical machinery built up over two millenia."[83r] The gross object of study is parsed by the formal system metaphor and is grounded and given a locus in terms of a theory of a modularized mind.

One cannot object to some such modularizing strategy. The not inconsiderable pay off is that it makes the gross phenomenon tractable and gives us a certain access.

And, in any case, how else might one proceed but on the basis of the conceptual material we have at hand? Moreover, Fodor suggests,[84r] in respect of cognitive abilities in general, that such modularization is the condition on there being the possibility of any explanatory study at all. He places this under the heading of a "First Law of the Nonexistence of Cognitive Science". What this "law" amounts to is the view that without some such carving of joints the gross phenomenon would be simply too complex to be dealt with; we would end up, in the linguistic case, with a law of the nonexistence of linguistics (in the same way, on Fodor's view, we are precluded from a theory of the central processor), thus returning us to roughly the place from where Saussure and Chomsky left off.

Unfortunately, however, unless one is to confuse methodology with verification, one cannot cite as confirming evidence for a modular approach that it is the only way we can presently come up with by which to attempt an explanatory account and so it must be right. Even less can one have confidence, as if on some sort of *a priori* grounds, in any particular (e.g. this) carving of the joints made on the basis of the conceptual apparatus that is presently to hand.

More to the present point, however, while not objecting to the hypothesis *qua* hypothesis, the issue is one of what might be the disconfirming evidence that could be adduced in falsification of that hypothesis. What is crucial, if we are to accede to a theory's truth claims, is that the theory-internal pressures that drive theory construction do not themselves supply a set of assumptions, such set as would constitute an unnegotiable dogmatism, that interpret the evidence so that that evidence is consonant with the theory and circularly confirms those very assumptions. We need evidence that is outside of the circle of theory construction. Otherwise the suspicion might be that all we are doing is characterizing the world in a manner that is consistent with certain prefiguring, modularizing and discourse-introducing concepts and then claiming the world to conform inherently to them, as if the world has to conform with our own interests: a case of conceptual necessity.[85] This might be because it does so conform, i.e. we have got it right, on the other hand, it might be that if some account allows itself to unquestioningly rely on some categorization, then it is not too much of a stretch to imagine that the results are going to be skewed in that categorization's favour: if your only tool is a hammer, then the world is liable to look like it's filled with only two sorts of entity: nails and non-nails.

4.3 Falsifiability.

> *In so far as a scientific statement speaks about reality, it must be falsifiable: and in so far as it is not falsifiable, it does not speak about reality.*
>
> (K. Popper, 1959).

> *You will find that there are many theories. This is always a bad sign.*
>
> (B. Silver, 1998).

While granting the practical need for a modular account, what we are insisting on is the requirement that the data are, in principle, able to disconfirm not only the hypothesized contents of the modules, but also the very demarcations that those modules propose. The hope, in respect of the content and so, implicitly, the boundaries of the modules, is that theory development in each will feed off theory development in the other in the manner of a sort of (dis)-confirming dialectic. So, for example, the postulation of some phenomenon as being due to pragmatic/performance factors - let's say an otherwise explanatorily powerful analysis is frustrated by some problematic data - will be properly tested and potentially disconfirmed by a pragmatic/performance theory. However, this is only going to be realizable on condition that some firm, falsifiable content is given to the pragmatics/performance module and that it does not simply act as some sort of carpet under which one might sweep problematic data, otherwise what one gets is not a disconfirming, but instead, a self-confirming dialectic.

It is precisely just such a problem which an account in terms of a competence/performance distinction gives rise to. If we accept that linguistic performance involves, or is integrated with, Fodor's "central processor" (and what constitutes central processing is resistant to theorizing: it is defined, on Fodor's account, as being more or less anything and everything, i.e. just the sort of thing one cannot have a theory of; conversely there is no obvious limit to the stories that can be spun about the central processor), then, on the one hand, we get to tell consistent and plausible stories - phenomena that are problematic can always be parsed so that the problem can be marked down to performance considerations - but, on the other hand, *in principle* we get unfalsifiable stories by virtue of the principled lack of theory about the central processor.[86]

The problem of potential inability of some claim to admit of disconfirmation becomes the more acute when one considers that the data are, in practice, restricted to

speakers' intuitions. Such intuitions are, as we have noted, in need of some interpretation and one is brought to inquire into the manner in which they are interpreted. The answer, in short, is that they are interpreted in a manner that assumes the posited modularization as a given, "as a preconceived idea to which reality [is made] to correspond", and so gets to be self-confirmatory of it.[87r]

4.4 Intuitions.

Like objects of study, data do not come as givens, ready labelled with indications of what they are data about, they have to be interpreted as being relevant to whatever phenomenon is under investigation. If the object of study for linguistics is taken to be the mentally realized competence grammar, the I-language, then the data need to be either directly (preferably) or indirectly about that competence grammar. The first problem is to find some data. Chomsky's move, and the standard working assumption, is, firstly, to take the status of native speaker to confer an entitlement to knowledge of a language,[88] and, secondly, to take it that this knowledge is reflected in intuitive judgements. This, however, does not get one very far when what one wants are data about the hypothesized competence grammar, for who is to say what cognitive resources are applied to in the making of such judgements?

> We ... do not know the psychological factors which determine the formation of such intuitions. It would be foolish to make linguistic virtue of psychological necessity by concluding that these factors are unimportant simply because they are unknown, but this is precisely what is done when linguistic intuitions are made the key to linguistic competence.[89]

In a similar vein, it can be, and has been, objected that intuitional judgements are, at best, a second-order linguistic phenomenon: they are "highly derived, artificial forms of linguistic behavior"[90r] that are, seemingly, only meta to what are pre-theoretically first-order linguistic phenomena, i.e. tokened strings; there is, after all, no obvious reason to exclude the possibility that "the behaviour of producing linguistically relevant intuitions may produce some properties which are *sui generis* and which appear in *no* other language behaviour."[91] Chomsky's initial assumption

was to brush such considerations aside and simply identify speaker-hearers' intuitive judgements with the hypothesized competence grammar:

> the grammar is justified to the extent that it correctly describes its object, namely the linguistic intuition - the tacit competence - of the native speaker.[92r]

However, is it not the case that before we can have confidence in this identification "an adequate linguistics should explain [and, one might add, adduce evidence to show] why it is that the intuitions of a speaker/hearer constitute data relevant to the confirmation of grammars[?]"[93] For, as Chomsky himself recognized, by itself "intuition is an extremely weak support."[94r]

The explanation offered is that the I-language is involved in the aetiology of linguistic behaviour, which behaviour includes the making of judgements of acceptability: it is the same knowledge resource that is applied to in all aspects of linguistic performance. What we are encouraged to is:

> The standard picture [which] views the theory of grammar as the theory of a context-independent resource - as, one might say, a clerk at a desk who is consulted from time to time on linguistic matters that come before the mind.[95r]

What is happening here is that the competence grammar is being posited in explanation of the ability to have acceptability judgements and it is just this very ability that is required if such data (the only practically available data, unless we return to the use of corpora) are to be directly relevant at all. It is the condition on being able to play psychological linguistic ball using intuitive judgements. It is also an instance of a certain circularity in theory construction; an instance of an assumption that is forced by the initial hypothesis in order to preserve the cogency of (and give content to) that initial hypothesis.[96] So, we need to ask what is the justification for the practice of taking "informant judgements ... as 'direct evidence' as to the structure of the I-language", just what is the evidence (other than theory-internal requirements) that intuitions give access to, or are even vaguely related to, some body of internally represented linguistic knowledge?

It is to be emphasized that raising this issue is not just a case of nit-picking, but is fundamental to Chomsky's psychologism, because if there can be shown to be grounds for serious doubts that intuitions can be construed as causally related to an

internalized competence grammar, if this construal of intuitive data can be shown to not make sense, then, while intuitions must be data about something or other (presumably, but not necessarily, directly linguistic[97]), they will not be interpretable as data about the putative object of study, the I-language, which is posited in explanation of our knowledge of language. Indeed, this would leave Chomsky's I-language hypothesis lacking any certifiable content, it would amount to no less than an "empirical apocalypse".[98r]

Returning to our question as regards the grounds that are adduced for the propriety of using intuitive judgements, Sells reports that "no justification for this practice has ever been offered."[99r] This, however, is somewhat overstating the case, for there is a perfectly cogent justification on offer which, while it does not excise the circularity we observed, does go some way towards making that circularity benign.

One removes the viciousness by pointing out, firstly, that this move of interpreting data to be pertinent to some posited construct is a standard manoeuvre in the sciences. In any case, how else might one proceed where one's starting point (and which point is what makes sense of the enterprise) is one of ignorance. To take one of Chomsky's examples, suppose one wanted to understand how it is that there are the phenomena of heat and light. The obvious direction to go is by pointing to the correlation of these phenomena with the presence of the sun. Of course, one then wants to explain how the sun generates this energy. One might posit thermo-nuclear reactions in the hidden interior of the sun and then, on that basis, take the light and heat experienced as data about those posited reactions. Such data can be taken as potentially disconfirming of any properties that may be postulated of those reactions. The objections raised by Levelt and Russell appear to be no more than arguing that one does not know how the phenomena are caused and so one cannot relate those phenomena to any putative cause, as if we are to be consigned, care of such scruples, to ineliminable ignorance. And after all - this is the second justification - what you end up with is an explanation where before we had none. Moreover, it is an explanation which justifies the assumption because it is the making of that assumption which allows one to make sense of the phenomena.

The point being made here is, essentially, a variation on Harman's theme of the inference to the best explanation:[100r] one is justified in accepting a statement (i.e. believing it to be true) because it is the best available explanation - the one that makes the most sense of the data.

Unfortunately, at this point the seemingly clear water gets muddied somewhat. Firstly, as a point of interest to be noted in passing, such considerations, (indeed

considerations that Chomsky explicitly encourages us to take on board[101r] in persuading us of the appropriateness of a realist construal of the theory,) are entirely consonant with a theory of truth which, rather than being compatible with a scientific realism (i.e. a correspondence-to-facts-of-the-matter view of truth), are much more at home with an instrumentalism (and a coherence, making-best-sense-of-things, notion of truth).

Secondly, again to be mentioned in passing, but also to be put down on our agenda for future discussion, is that Harman's principle is not unproblematic, but:

> needs qualification, since sometimes it is unwise to ignore the antecedent improbability of a hypothesis which would explain the data better than others.[102r]

We take up the question of the possible antecedent improbability of Chomsky's explanatory constructs and manoeuvres in chapter IV.

Thirdly, and more to our present point, the benignity of the circularity comes into question when it is recognized that, even if we accede in the assumption that the mentally represented grammar is the primary basis for the ability to make judgements of well-formedness, those judgements cannot be unaffected by other factors. The standard examples cited are cases of multiple centre-embeddings and garden paths, (both of which sentence types are judged unacceptable but which are grammatical,) and ungrammatical strings that are generally deemed acceptable.[103] The standard explanation of this discrepancy between intuition and grammaticality makes use of the recognition that judgements are performance phenomena and as such are affected by "the intrusion of numerous other factors",[104r] in other words, the intrusion of the central processor. The upshot is a weakening of Chomsky's (1965) identification of intuitions with tacit competence, instead we are encouraged to the view that taking intuitions:

> as "direct evidence" as to the structure of the I-language ... is only a tentative and inexact working hypothesis, and any skilled practitioner has at his or her disposal an armory of techniques to help compensate for the errors introduced.[105r]

What this "armory of techniques" is required to do is to be able to correctly filter out the intruding other factors, i.e. to parse and apportion causes to the data. However, to do this is it not necessary to specify, in advance, just what these other

factors/causes are, and what their effects might be, independently of what they are required to be such as to preserve the theory? Without such specification (and I know of none - we are back here with the need for a worked out performance/pragmatic theory; a theory of the, for Fodor, non-theorizable central processor[106]) what seems to be in operation is a meta-theoretical implement that one might dub Occam's broom (it is found, unhygienically, in the same cupboard as the razor, perhaps): it allows one to sweep under the carpet any unpalatabilities and so keep simple what is to be explained. However, the notion of what is simple here is defined wholly in terms of the theory that one is positing. Ultimately what you get to sweep under the carpet are unpalatable data, and so what you get to preserve and make unfalsifiable is, firstly the analysis that is prefigured by one's notation and, secondly, the very modularization that has been introduced by way of the initial formal system metaphor: these other factors being precisely and negatively defined as those phenomena that cannot be handled in terms of the properties and parameters of the formal systems that are posited.

Again a justification might run along the lines of the explanatory success that is afforded, what we get is an account and an account which is, apparently, coherent. Of course, one does get to tell such a story, but then with a reasonable amount of imagination regarding one's "armory of techniques" one could hardly fail to.

4.5 Theory-internality and "closed circuit functioning".[107]

In general terms, the danger that we have been remarking on is that of theory construction (as regards both the task of extending the coverage of some framework and as regards the disconfirmation of analyses) engaging in a fallacious, hermeneutic circle, or, for aficionados of Winnie the Pooh, in a "Woozle hunt."[108] It may be recalled that what Pooh failed to notice in his tracking down of the Woozle in the snow was that the evidence for Woozles - and evidence for their properties - was what had been originally put there by his own paw-prints and then added to as he went round in circles looking for them. A further, related, cause for disquiet is the observation of the ease with which recalcitrant data are, on occasions, handled. The following provides some illustrative examples.

A framework achieves a certain coverage over some fragment. When one meets problematic or apparently disconfirming data one argues that the coverage already achieved licences one's continuing in the assumption of the constructs and

analyses posited. One can get round the disconfirmatory obstacle by citing, in Chomsky's phrase, "the intrusion of numerous other factors". In short, the maintenance of the theory, on the grounds of the generalizations that the framework affords, forces a saving assumption; making the assumption, not surprisingly, allows or interprets evidence to be confirmatory of the theory; the resulting explanatory success gets to be confirmatory of the theory and the assumption. When it is pointed out that there is little or no evidence for the assumptions that have been made, external to this circle, one cites the explanatory success achieved. Either that, or (a non-exclusive "or") one hives off problems onto some other field (standardly performance - a corollary of the formal system metaphor), or takes refuge in the idealization or "abstraction from mechanisms" that are claimed requisites of the enquiry, or one characterizes the discrepancy between one's linguistic theory and what some theory-external perspective[109] tells us, for example psycholinguistics or evolutionary biology, as either a problem for that external theory or "a mystery". The implicit claim here - that linguistic theory is confirmed beyond disconfirmation by these other perspectives (i.e. precisely such perspectives, such other points of view that, by taking us outside of the circle, might give confidence in the reality of the theory's constructs) - is made on the basis of no more than the explanatory success achieved in this theory-internal and circular way.

Much this set of problems and manoeuvres is in evidence in the standard pattern, replicated innumerably in the literature, for papers arguing for one's framework on the perfectly cogent basis of its ability to provide analyses of the data, to make correct predictions, in short to achieve coverage. Accordingly, one argues *pro* or *con* competing frameworks or analyses in the same manner. The schema of the argument goes somewhat as follows:

a) Here is construction type A and the relevant data regarding acceptability judgements.

b) Here is what Tom, Dick and Harry propose in lines with Tom's, Dick's and Harry's frameworks respectively.

c) But here is the puzzle for Tom, for Dick and an apparently disconfirming clincher *contra* Harry.

d) Here is the general outline of My (variation of the) framework.

e) Look, if we do this, this and this (a feature here, an assumption there), then we get all the data out.

f) (If it cannot be avoided.) True, there are one or two puzzles or problematic cases remaining.

However, there are a couple of strategies that can be applied at this point:

i) Dispute the data. Liberal use of "clearly" or some similar phrase is recommended to bolster your side of the discussion.[110] Alternatively, be inventive with "?" and "*" as in "?*", "??" and the permutations thereof.[111]

ii) Concoct some more or less plausible story to account for the acceptability of what you want to be an ungrammatical string (or the unacceptability of a grammatical string) in terms of performance/pragmatic (any other you can think of) factors.[112]

This is, of course, not the end of the story, for although My theory does a fine job as regards construction type A, we can justifiably defer our acceptance of My theory until this success is replicated across a sufficient range of construction types. In the meantime, the position, in practice, is that Tom's theory, in the case of construction type B, does a much tidier job than My theory, as does Dick's in the case of C-type constructions. Furthermore, Harry's theory, although it has certain problems, beats all-comers in respect of construction type D. So we still have no good grounds upon which to prefer one account over another. One might be tempted to invoke some criterion regarding the extent of coverage that a theory achieves, however, this is at least as likely to be a function of the state of development of a framework as it is to be a mark of the theory's ability to be regarded as true.

What these observations infer is the open-ended nature of the task of deciding between grammars in terms of coverage. This is the more acute when one considers the question of what is intended by the notion of coverage: suppose grammar X and grammar Y both, over some subset of the phenomenal range, a fragment of a language, make successful predictions, then we are left with the question of how a decision might be effected. One may point out that a decision must be deferred until both grammars can be compared in respect of their ability to make correct predictions across the whole phenomenal range, but in practice not only are there no grammars on offer that even come close to achieving this range, but also it is not clear whether this notion is itself practically definable and so usable given "the vastness of natural language": what has to be accounted for is open-ended. This is hardly surprising when one considers that a standard construal of what constitutes the object of study for

linguistic theories is that it is a set of sentences, which set has indefinitely many members. The general point being that there can be no provisional, let alone final, decision made for or against any linguistic theory solely in respect of a theory's coverage. What these observations amount to is one more reason, to add to our observation of the insulated circularity of theory construction and disconfirmation, to defer any realist construal of a generativist linguistic theory.

5.0 Realism and skeuomorphs.

To recap a little: our observation is of an initially metaphorical relation between some conceptual apparatus and the phenomenon it is intended to explicate. In this linguistic instance, the originary metaphor is the formal apparatus that institutes a domain of discourse. Our previous discussion, in paralleling the role of formal calculi in linguistics with the role of mathematics in the sciences in general, has reminded us that formal models do not, by virtue of their modelling ability, thereby necessarily, by some form of Galilean conceptual necessity, articulate the inherent nature of the object modelled, nor do they necessarily demarcate the world in respect of the way the world is itself carved at joints. This observation warns of the need, on the one hand, for proper warrant before the metaphoricity can be excised and exchanged for a realism, and also warns of the need to beware of pre-conceived ideas and assumptions that are brought or smuggled in by way of the conceptual apparatus and, as it were, projected onto the object of study as apparent givens.

To excise the initial metaphoricity, criteria need to be adduced, the meeting of which would provide confidence that the identified object of study, (in so far as it is taken to correspond in its properties and parameters to the properties and parameters of the modelling calculus,) corresponds in virtue of itself and not in virtue of being projected by the assumption of the conceptual apparatus that figures it and which supplies our conceptual spectacles. In short, we need grounds for confidence that a postulated competence grammar and its constructs are not fictions, that the set of linguistic entities that are postulated exist as real, theory external objects (as *illata*) rather than (to reprise Saussure) as no more than objects created by the point of view of the formal system metaphor, that is as no more than projections (*abstracta*) of the terms of the formal systems employed in theory construction.[113] We take up this issue in chapter II.

The general suspicion that we have raised is that the internality of theory development and verification we have discerned is undermining of any realist construal. What we get is a discourse and methodology that has many of the characteristics of what Popper terms "pseudo-science", a primary characteristic of which is resistance to any possible falsification.

Picking up on the latter of the above two issues, one of the features of linguistic theorizing is its top-down nature and the carrying down of constructs posited in analysis of some fragment as assumptions that, when the coverage of some framework is extended, direct further hypotheses of further constructs. A difficulty is that, on the one hand, there is, apparently, built-in methodological licence to explain away disconfirming data when they interfere with some perceived explanatorily powerful generalization (itself a function of the particular economy of information of the formalism) that otherwise would be lost. On the other hand, evidence for these constructs is not available external to the analysis which postulates and interprets them: such constructs are unobservables and, by virtue of the restricted, range of data (acceptability judgements and little else), radically unobservable except by virtue of the spectacles provided by the notation system; what we end up with is a Woozle hunt. One can arguably "observe" sentences, but the theoretical apparatus that is recruited to the job of accounting for the judgements of acceptability of those sentences involves, according to framework, a series of sub-sentential theoretical constructs, move α, greed, unification of features etc., which are elements of the calculi employed and which, when predicated as real of the object of study, are unobservables generated by the choice of calculus. It is in this respect that "notation systems [i.e. calculi] create analyses."[114r] The misgiving is over whether those analyses, and the constructs which enable them, can be discerned (i.e. made available for possible disconfirmation) independently of the framework which generates them, that is as real properties of natural language as opposed to real properties of whichever framework is the vehicle for representation.

What is discernible in this is the general pattern of a realist claim: that properties and parameters of some modelling apparatus are predicated of (or transposed onto) that with which it is in a modelling relation. It is this modelling relationship that is the hypothesis to be disconfirmed. What has been further suggested in our discussion is that certain properties, which are brought in by way of the formal system metaphor, remain as base and unverified assumptions that, if the metaphoricity is warranted to be excised, are required to be the case of the object of study. These properties constitute, in terms of our commercial metaphor, the cost of

the formal system metaphor. At this juncture we are concerned with merely noting what these properties are. In the following chapters we intend to further substantiate the suitability of a non-realist/instrumentalist construal of theories in Chomsky's generativist paradigm and to show that the following unverified assumptions (transpositions, or corollaries of the transposition, of properties of standard formal systems onto putative objects of the real world) either cannot be the case or cannot be the case of an object of study that is taken to be a psychological object:

a) Intuitions construed as accessing information realized by a mentally represented body of declarative knowledge.

b) The modularization of the gross phenomenon along the axis of a competence/performance distinction.

c) A characteristic of generative grammars is that they generate a denumerably infinite set of strings, a subset of which strings are indefinitely long. Transposing this property onto the putative object of the real world that we are investigating requires that the set of sentences of a natural language, i.e. those generated by the I-language, is, correspondingly, denumerably infinite and, consequently, is "transcendent" of any particular set of events (i.e. linguistics is concerned not with linguistic tokens (corpora), but, as is the (usually implicit) consensus, with linguistic types). The set of sentences of a language is conceived of as, and assumed to be, given and fixed by the properties of the I-language.[115]

d) A corollary of this assumption, and a property of generative grammars *qua* mathematical systems, is the property of bivalence: for any random string composed of terms of a language, a generative grammar provides a decision as to the inclusion or exclusion of that string in the set of strings in the language; a string is either grammatical or ungrammatical, there is no middle term.[116]

These transpositions, where they are uncorroborated, may be placed under the heading of skeuomorph effects.[117] The dictionary entry for *skeuomorph* has it as i) an object or feature copying the design of a similar artefact in another material; ii) an ornamental design resulting from the nature of the material used or the method of working it. Such observations, and the perspective from which they are made, are part and parcel of instrumentalism in the philosophy of science. This is the view that scientific theories are not capable of literal truth or falsity because the putative real and independent world is not available independently of the conceptual scheme which figures that world. Theories are to be taken as no more than fictions that are

instrumental in providing predictions of events, but the terms and constructs that are involved in this predictive success are not to be taken as corresponding to real entities and so are not to be included in our ontology. Inability to assuage the misgivings of there being a skeuomorph effect (that what is claimed to be the real world case is what is put there by illicitly quantifying over entities such as would interpret the terms and relations of some formal, theory-constitutive apparatus) would constitute an obstacle to construing such theories and their constructs as corresponding to the real linguistic facts of the matter.

The misgivings that we have aired are, surprisingly perhaps, echoed by Chomsky:

> [The] conclusions reached might prove to be a kind of artifact, a result of our methods of investigation and theory construction, not properties of the real world that we are investigating.[118]

Such worries concern the instrumental role of any conceptual apparatus: the worry, to recap, is not that such apparatus is instrumental in providing hypotheses about some object of study, but that such apparatus is instrumental in constructing an object of study and in providing its properties *in lieu* of some independently existing object of the real world which is the putative object of enquiry, and which object can not be discerned independently of the vehicle which expresses the theoretical claims. If these suspicions can be substantiated, then we will have shown that realism toward linguistic theories which share these certain traits will be misplaced. To investigate this possibility it is, firstly, worth enquiring, albeit somewhat summarily, whether a scientific realism is itself, in general, misplaced. If it is, then our raising of the issue of some skeuomorph effect would be redundant and no more than stating the unavoidable.

Footnotes.

[1] The spectacles metaphor is borrowed from Wittgenstein (1958, section 103).

[2] S. Gould (1983, pg. 363), cited in G. Lakoff (1987, pgs. 119 - 120).

[3] What this amounts to is an implicit predilection for a "God's truth" realist metaphysics. Arguably, such a realism is also implicit in much of our everyday discourse - Putnam terms it "a natural impulse" (1981, pg. 74). The view that there is no one final correct taxonomy is consonant with a "hocus-pocus" instrumentalism. More on this below.

[4] One historian of science (D. Boorstin, 1983, pg. 290 and pg. 294) suggests that a prerequisite for the development of modern science was that common-sense apprehension of phenomena be acknowledged as defeasible: "Modern Western science takes its beginning from the denial of ... commonsense axioms". Even more forthright, in respect of the history of cosmology, is Hoskin (1997, pg. 32): it is "the heroic saga of the hard won rejection of the patently true in favour of the absurd". The examples cited, by both authors, being the obviousness of the earth's motionlessness and centrality vis-à-vis the sun, stars and planets.

[5] A. Einstein cited in J. Katz (1985, pg. 173).

[6] P. Churchland and T. Sejnowski (1989) in W. Lycan (1990, pg. 226).

[7] The term "irreduction" is from B. Latour (1988). Sellars' phrase is from W. Sellars (1956).

[8] B. Smith (1996, pgs. 77 - 78). Smith also supplies the "commercial metaphor" (somewhat amended).

[9] F. de Saussure, (1916, pg. 24 - 25).

[10] The latter quotation is from N. Chomsky, (1980, pg. 217), the former from N. Chomsky, (1992, pg. 102) cited by Burton-Roberts and Carr (forthcoming).

[11] F. de Saussure (1974, 3).

[12] *Ibid.* 14.

[13] N. Chomsky (1964, pg. 23).

[14] N. Chomsky, (1964, pg. 51).

[15] E. Post, (1936), this being furthered by Harris' and Chomsky's own developments in the fifties.

[16] One may wonder, given the "muddle" of the linguistic, whether the centrality of this fact is really "phenomenologically obvious", a pre-theoretic given. In fact, it seems, it isn't: Chomsky has noted that, obvious or not, positing a generative grammar in explanation of creativity was misconceived (see Parret, 1974, pg. 28): "a confusion of competence and performance". What recursion gives is the *means* for creativity, it is not itself an explanation of creativity. The recognition of this has led to the shift to the present centrality, for an explanatory linguistic theory, of the facts of acquisition. The cynic might observe that what is the central fact seems to be suggested by what you look like having the means to explain.

[17] T. Moore and C. Carling (1982, pg. 62). They are, arguably, guilty of making rather more of the observation than they ought; they appear to take the hypothesis of a formal system as somehow reprehensible in itself. Plainly, it is not: one needs further argumentation to the purpose of showing that formal systems, as presently conceived, are simply not up to the task of modelling the facts (Lakoff takes up this further line of argumentation in indicating that standard formal systems cannot do justice to actual natural language categorization schemata (G. Lakoff, 1987).) One has to make use of some conceptual means in formulating theory. What would be reprehensible, and we argue is in fact reprehensible, would be to give way to the temptation to take the appropriateness of the formal system metaphor as some sort of given. However, Moore and Carling's contention is supported by an argument to the effect that linguistics is insufficiently clear about what constitutes linguistic objects. Such clarity, they argue, is a precondition for the appropriateness of deductively formulated theorizing (i.e. theory construction articulated by means of some (quasi) formal system). As will be noted, fairly well the same point will be made here except coming from the other direction: having posited certain linguistic constructs, linguistic theories are unable, practically, to adduce proper criteria of identity for those constructs.

[18] F. de Saussure (1916, pg. 23).

[19] G. Lakoff (1987, pg. 228).

[20] The quoted phrase is from Suppe (1977), it may be construed as having more or less the same content as Chomsky's (1980, pg. 11) observation of a "Galilean style" of theory construction in the natural sciences.

[21] Compare Lakoff's account (G. Lakoff, 1987, pgs. 219 ff.).

[22] For example, the set-theoretic relation of inclusion, "A is a subset of B", is the same as the logical relation of implication, "if A, then B".

[23] J. Barrow (1993, pg. 111).

[24] See D. Hilbert, "On the Infinite" in P. Benacerraf and H. Putnam, (1983).

[25] Cited in S. Dehaene, (1998, pg. 243).

[26] L. Coutarat, cited in J. Barrow, (1993, pg. 114).

[27] Compare Pollard and Sag's account (1994, pg. 6 ff.).

[28] R. Boyd (1979, pg. 363). The physicist Richard Feynmann, in countering objections to the use of such metaphors, remarks:

> People often complain of the unwarranted extension of the ideas of particles and paths etc. into the atomic realm ... there is nothing unwarranted about the extension. .... It is the only way to make progress. (R. Feynmann, (1965, pg. 164).)

More generally, this role of metaphor, in the guise of "mapping across domains" (Carey and Spelke, 1994) or "representational redescription" (Karmiloff-Smith, 1992) or "transformation of

conceptual spaces" (Boden, 1990) is argued to be the basis for human creativity: it is why we are so clever. For Gardner (1983) "wisdom" is just such an ability to build connections across domains, prime exemplars of which are our use of metaphor and what Fodor (1985, pg. 4) notices as our "passion for the analogical". (These are not entirely novel remarks: Aristotle's discussion of metaphor suggests that its use constitutes evidence of superior intellect.) It is the correlative holism and lack of encapsulation of the "central processor" that Fodor (1983) suggests is sufficient to defeat an attempt in its account. For a less defeatist view, and a considerable degree of fundamental consensus, see the above authors and Sperber (1994).

[29] J. Cohen and I. Stewart (1994, pg. 23).

[30] A typical example being Searle's Chinese room argument (J. Searle, 1980).

[31] The issue of quite what the phrases "facts of the matter" and "inherent to the object" intend will be taken up in chapter II.

[32] See, for example, Carr, (P. Carr, 1990) from whom the phrase is borrowed. Much the same can be discerned in Newmeyer's defence of the Chomskyan/generativist paradigm (F. Newmeyer, 1983). Such thinking is a variant on the theme of Harman's notion of the inference to the best explanation (G. Harman, 1965).

[33] N. Chomsky, (1980, pg. 109).

[34] H. Putnam, (1989, pg. 214).

[35] R. Robins, (1971, pg. 41), cited in C. Hutton, (1993, pg. 167).

[36] *Ibid.*

[37] This assumption is, of course, what underlies referential/denotative theories of meaning and, ultimately, the whole contraption of possible worlds in Montagovian semantics. In the case of the mathematical example, the assumption founds a Platonist/realist view of mathematical objects.

According to Kay (1979) (cited in Lakoff, 1987, pgs. 121 - 125) this assumption is part and parcel of a folk theory of language meaning. (Derrida's notion of "logocentricism", "the traditional order of priorities of language, meaning and truth", points in a similar direction.) In brief, the folk theory is to the effect that words and thought fit the world, that:

> there is a world independent of our talk and ... our linguistic expressions can be more or less faithful to the non-linguistic facts they represent. (P. Kay, 1979, pg. 37)

[38] The reader will, perhaps, recognize the phrase as a description of certain conclusions of the Minimalist program (e.g. N. Chomsky, 1995, pg. 385). Interestingly, Chomsky has also used the phrase as a description of what results from taking a Platonist view of the linguistic (1980, pg. 29 - 30). In that context the phrase is used as a stick with which to beat Platonists. We will use it below (chapter IV) as a stick with which to beat Minimalism.

[39] H. Putnam, (1980, pg. 482).

[40] B. Smith, (1996, pgs. 80 - 81). It may be worth noting that Smith's book grew out of concerns with the nature of the objects and constructs of the discourse of the computational sciences.

One can provide a Kuhnian interpretation to these remarks: Kuhn's (1962) central point is that work within a scientific paradigm depends upon, but is not concerned with considering, its set of foundational assumptions. Smith's point, something of a counterpart to our prefatory quotation from Heidegger, is a request that one be aware of those paradigmatic assumptions, which assumptions, in our terms, provide the initial epistemic access onto some area of enquiry.

[41] See N. Chomsky (1980, pg. 11). The adjective was originally coined by Husserl.

[42] Cited in S. Blackburn (1994, pg.152). Such a view is representative of a long tradition from Plato ("God is a geometer") to, contemporarily, Roger Penrose (see below). The Pythagoreans held that "all things that can be known have number; for it is impossible for a thing to be conceived or known without number." (Philolaus of Croton, reported in J. Barrow, (1993, pg. 252)).

[43] R. Penrose (1995, pg. 415).

[44] E. Wigner, (1960, pg. 527). He remarks that "the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and there is no rational explanation for it."

[45] Plato: *Timaeus*, (section 90). For a rehearsal of this position in a somewhat less quaint and more evolutionary idiom, see J. Barrow (1993): in short, maths is real of the world, it has really a mathematical substructure, and:

> if our minds have derived a special mathematical facility from the real world, it is likely that they have done so as a result of an evolutionary process which has selected for those mental representations of the world because they most faithfully represent how the world truly is. (pg. 263.)

It is unlikely, however, that evolution, as it is understood on the Darwinian model, can be held responsible for our evolving concepts that provide such a transparent window on the world, not unless selectional advantage can *only* be achieved on the basis of having representations of the world that are true. Truth from an evolutionary perspective is more likely to approximate to a pragmatist's conception: what are selected for are representations/theories that work and representations/theories that work may do so despite not being true on a correspondence notion of truth. (see footnote 25, chapter V, pg. 211).

[46] N. Chomsky, (1980, pg. 9). Here "chance" is doing the same job for Chomsky as evolution does for Barrow (see footnote 45).

[47] G. Lakoff (1987, pg. 262). It is not too much of an interpretative stretch to suggest parallels with Husserl's phenomenological project with its suspension of judgement and of ontological commitment (*epoché*) to the objects that are posited as the referents of discourse conducted from the perspective of the "natural attitude" (E. Husserl, 1931, pg. 111). According to Bell (1991, pg.164,) and compare

footnote 37, this "natural attitude" (compare Putnam's "natural impulse") involves "the belief that, on the whole, the world is very much as we perceive it and conceive it to be."

One may point to Kant's "Copernican Revolution" as the intellectual precursor of this change of perspective with its suspension of belief in the transparency of mind/language to "things-in-themselves" and a concomitant foregrounding of the *a priori* categories by which we apprehend the world.

Putnam's "internal realism" (1981) may be considered as a latter-day Kantianism. It is a position that is more or less implicit in the stance that is being elaborated in our discussion. In very brief, internal realism holds that there is an external world with all its objects, but that world is epistemically accessed only by way of the conceptual apparatus which is available to us. There is, consequently, and contrary to a metaphysical realism, "no God's Eye point of view...; there are only various points of view of actual persons reflecting various interests and purposes that their descriptions and theories subserve." (pg. 50) Contrary to an idealism, i.e. the view that the world is no more than some fiction that is spun out the contents of our minds:

> Internalism does not deny that there are experiential *inputs* to knowledge [i.e. care of the impinging of the external world]; knowledge is not a story with no constraints except internal coherence; but it does deny that there any inputs *which are not themselves to some extent shaped by our concepts.*"(pg. 54).

[48] For a similar view in respect of "the unreasonable effectiveness of mathematics", see S. Dehaene (1998, pgs 249 - 252).

[49] E. Wigner (1991, pg. 540).

[50] L. Wittgenstein, (1961, Proposition 2.161).

[51] P. Hacker, (1986, pg. 60), cited in C. Hutton, (1993, pg 170).

[52] Cited in B. Kosko (1994, pg. 3). A similar scepticism as to the precise fit between mathematics and the structures of the physical world is warranted by the fact that even in the case of as well-understood a theory as Newton's account of gravity the mathematics is not up to predicting precisely the gravitational effects of more than two separate bodies (more precisely, there is no formula that solves/predicts the motion of two or more bodies exactly). This was proved by Poincaré in 1889. To similar effect, Dehaene points out that, in general, "mathematical models rarely agree *exactly* with physical reality" (S. Dehaene, (1998, pg. 251)).

[53] This latter is the conclusion of the Löwenheim-Skolem theorem. See S. Blackburn (1994, pg. 352).

[54] A. Church, (1932), cited in J. Barrow, (1993, pg. 16).

[55] M. Kline, (1980, pg. 521). Barrow (1993, pgs. 8 - 20 and 154 - 156) remarks on the intellectual shock that the discovery of non-Euclidean geometries caused (for "the idea of axioms was for the first time divorced from physical reality, ... they were no longer aspects of that physical reality.").

[56] This (weak) equivalence was proved by Bar-Hillel, Gaifman and Shamir (1960). For a discussion, see L. T. F. Gamut (1991, vol. II, pgs. 95 ff.).

Other examples of such "extensional equivalents" in mathematics and other fields are not difficult to find. For example, in classical mechanics, there are two ways of using mathematics to determine the trajectories of particles moving under the force of gravity, either one may use differential equations or a variational principle.

[57] J. Barrow, (1988, pg. 557).

[58] L. Wittgenstein, (1958, section 131). By "model" Wittgenstein is referring to "our clear and simple language-games ... set up as objects of comparison." I am taking such to be more or less equivalent to the metaphors alluded to above which institute discourse and theory.

It is interesting to compare these remarks with our previous quotation (pg. 30) from the *Tractatus*. One might be tempted (a temptation to which I will succumb) to point to these quotations as paradigmatic of both the continuity in Wittgenstein's concerns and of a central shift in his thought between the early and later work.

[59] "Fregean" in respect of the idea that permeates Frege's project of uncovering the real forms of language, some prefiguring calculus, that underlies the apparent surface muddle, the "amas confus" of Saussure's phrase. This is a project that goes back, through Leibniz amongst others, to at least the middle ages. It is generally accompanied by some notion of a universal language (see Robins (1967, pgs. 85 ff)).

[60] This method of theory construction is distinct from that which prevailed in the years of American Structuralism. Rather than the structuralist bottom up series of inductive generalizations being made piecemeal from and through the systematization of the data, instead the method is primarily top down, the calculus being in the manner of a general hypothesis that is to be disconfirmed in relation to its ability to model natural language strings, and hence to predict their acceptability. See N. Chomsky (1975a). For an elegant summary see J. Katz (1996).

[61] W. V. O. Quine (1976, pg. 40).

[62] R. Hersh (1998, pg. 167).

[63] R. Carnap (1937, pg. xiii).

[64] W. V. O. Quine, (1979).

[65] R. Hersh, (1998, pg. 171).

[66] J. Higginbotham (1987, pg. 126). An observation, consonant with this remark, is that "the search for a linguistic *framework*, as opposed to a substantive *theory*, that has just the right expressive power is ... misguided." (D. Johnson and L. Moss, 1994, pg. 539.)

[67] Examples of this style of theory development can be found in any of the standard text books as the manner of exegesis of the theory; for example, Haegeman (L. Haegeman, 1991).

[68] C. Hutton (1993, pg. 177).

[69] P. Sells, (1985, pg. 78).

[70] S. Stich, (1985, pg. 133).

[71] See W. V. O. Quine, (1972). The point is that where there is radical inability to select between theories then there is nothing of which those theories may be contentfully said to be true.

[72] Two grammars would be notational variants if they referred to all and only the same entities and relations but did so using different symbols, for example simply substituted "S", "NP", "VP", ... with "Σ", "NΠ", "ςΠ".... This is not problematic. What is problematic is where two or more grammars, over the same fragment of a language, achieve identical coverage but where the equivalence is only in their specifying the same set of sentences, where, for example, the constituents they specify differ and so require different models. For example, compare a categorial grammar and GB both with the same coverage over some fragment. Are these notational variants? (We take up, in chapter IV, Chomsky's addressing of the question by way of his three posited criteria for adequacy.)

[73] N. Chomsky, (1980, pg. 100).

[74] Providing an analysis involves positing a set of entities, i.e. the model (of syntactic objects) that interpret the terms of the formalism. Different analyses involve different models, corresponding to the different economies of information of various formalisms. But, whence these different models? For are they not supposed to be supplied by, in the sense of inherent in, the object of study? Are they not supposed to supply the content (meaning) of the symbols independently of the symbols. To requote Putnam: "Models are not lost noumenal waifs looking for someone to name them; they are constructions within our theory itself, and they have names from birth." (1980, pg. 482). Models (i.e. for present purposes the syntactic objects that interpret the terms of a calculus) are not things that are found in reality, but are what is put there.

Adam, goes the illustrative joke, after a hard day of naming things in Eden, goes home to Eve and his supper. As they are settling in to the hors-d'oeuvre a zebra walks past. "Oh, by the way, I decided to call that a 'zebra'". "What on Earth for?" asks Eve - she's a little tetchy - perhaps still bewildered by a not yet completely interpreted world. "Why do you think?" says Adam, "It just looks like one."

It turns out, by the way, that according to cladistic criteria "there is no true biological category that consists of all and only zebras." (J. Gould (1983), reported in Lakoff, (1987, pg. 119)).

[75] C. Hutton, (1993, pg. 174).

[76] N. Chomsky, (1980, pg. 100).

[77] A rejoinder to these remarks might point to the abstraction of the enquiry from instantiating mechanisms. We take up the issue in detail in chapter IV.

[78] In illustration of the notion of an epiphenomenon, an example (Hofstadter's) is that of a steam engine and its whistle. Imagine one wanted a general theory of the engine, one might address the problem by attempting some account that directly addresses the perceived behaviour of the whistle. It

is certainly something to be explained but, in hindsight, one recognizes that an attempt at a theory of the whistle *qua* whistle would be a mistake. Rather, it is a piece of perceived behaviour that is not directly specified for, it is not something that the engine is designed explicitly to produce, instead, it is an incidental by-product of the overall system, something that falls out of the design, but is not directly referred to by the design. Thinking of this design as a set of rules, the whistle "is not built into the rules, but is a consequence of the rules" (D. Hofstadter, 1979, pg. 308 - 309), it is not something that needs to be addressed on its own account.

To take a linguistic example, Berwick and Weinberg's (1984) functional explanation of subjacency reconstrues the set of subjacency phenomena as falling out of the actions of the (modified Marcus) parser and as a consequence rather blurs, indeed makes problematic, the standard competence/performance distinction. (A more recent proposal, in a similar style, i.e. "a theory of performance acts as a filter on possible linguistic representations", is made by Ackema and Neeleman (ms, 1998). Their paper is in attempted account of the disparity between the incidence of leftward and rightward movement, while preserving the symmetry (contra Kayne (1994)) of X-bar theory.)

It will be noted from these examples that the notion of an epiphenomenon is related to the computational distinction between procedural and declarative knowledge: a distinction between what is explicitly referred to by some program and what is implicit. For example, a program may define and refer to some strings of symbols as "sentences" which are subsequently manipulated by certain operations. One might think of "sentences" as being "real" to the computer that runs the program. On the other hand, a different but functionally equivalent program, may have the same output but with no explicit reference to the same strings of symbols as "sentences". Such a program has no "knowledge" of any entities as "sentences". Rather, "sentences" are what the program does, they are epiphenomenal, "a global consequence of how the program works" (Hofstadter, 1979, pg. 363).

[79] N. Chomsky, (1965, pg. 9).

[80] See C. Hutton, (1990) and N. Burton-Roberts (1994).

[81] Kempson (forthcoming, chapter 8) notes that despite the increasing recognition of the centrality of the phenomenon of underspecification of natural language content in recent years (a phenomenon which makes at least problematic the appropriacy of the formal language metaphor - an issue which we take up in chapter VI), "linguists have maintained the concept of a formal system" and that:

> Even in pragmatics ... the formal-language metaphor is sustained in so far as it is assumed that the grammar articulates some form of complete structure associated with a sentence string, subject to some form of semantic interpretation, with the remit of pragmatics being to explain how such information provides the basis for the very much richer modes of interpretation that are available in discourse.

[82] In making these remarks one comes across a point of connection between Anglo-Saxon and continental idioms - recall that we are, after all, engaged on an "Anglo-Saxon deconstruction" of the generativist paradigm. The relevant terms of continental art that give a perspective on the matters with which we have been concerned being Derrida's notions of "logocentricism" and of "supplementarity".

Derrida diagnoses logocentricism (compare footnote 37) as the *mythos* that underwrites the western philosophical project - as a thinking that attempts to get into a "pure, self-present contemplation of truth", it expresses a "desire to fix an origin for truth and knowledge", where truth is understood as the world as it is in itself. In other, Anglo-Saxon, words, truth is constituted by the model that interprets language. Language is a sort of supplement, a window on the world that is both necessary - we need some means to view the world - and *de trop* - because it defers the self-presence of the world.

In this light, language use, the exceeding of these origins, the extended deferment of the self-present, i.e. "pragmatics [, and here we have an Anglo-Saxon speaking,] is taken to be peripheral, and of secondary interest, since it is not concerned with having anything to do with objective reality" (G. Lakoff, 1987, pg. 171). Rather it is a detour from that reality, a reality which is the province of semantics insofar as semantics deals with matters of truth and ontology.

A symptom of the philosophical distrust of the supplementarity of language use is found in attitudes to, for example, metaphor, "a figure whose workings could always be explained by reference to some other, more reliable, or epistemologically privileged kind of language" (C. Norris, 1987, pg. 203). "Metaphor ... is determined by philosophy as a provisional loss of meaning, ..., a certainly inevitable detour ... with its sights set on the circular reappropriation of literal, proper meaning." (J. Derrida, 1982, pg. 270.) (Compare our own use of the notion of a theory constitutive metaphor as a hypothesis that has to be authenticated by reappropriation to a literal, realist meaning.)

Derrida suggests that the logocentric/semantic project is undercut by a "double logic" of the supplement. To say that x is a supplement of A is to say both that x is external and internal, is to say both $x \subset A$ and $x \not\subset A$. (Consider a volume that is a supplement to the OED. On the one hand one may regard such as no more than an optional extra: if one does not have the supplement one can still claim to have the complete OED. On the other hand, there is a proper sense in which the OED is not complete without its supplements.)

If we apply this logic of the supplement to the peripheral/external status of pragmatics/performance one might not be surprised to find, as we will suggest in chapter VI, that what has been intended to be non-originary and peripheral has to be reassessed as integral and originarily constructive of the nature of natural language.

[83] S. Levinson, (1983, pg. 145).

[84] J. A. Fodor (1983).

[85] A charge that seems to be extraordinarily pertinent when laid against "the modularity theory gone mad" of some evolutionary psychologists. See, for instance, Tooby and Cosmides (1992, pg. 113) who give an "incomplete" list of seventeen modules, including one for something called "effort allocation and recalibration" and, as perhaps a challenge to our imaginations as to what might not be included, break off triumphantly with "and so on!" - exclamation mark, indeed.

[86] These remarks are, arguably, not vitiated by Sperber and Wilson's (1986) theory of the central processor. Their theory is an attempt to outline the principles of the central processor's operations. It is regarded as an inferential machine, with a demonstrative logic that, designed on a least cognitive effort for most cognitive gain basis, makes sense of input in relation to manifest and accessible premises. The problem is one of what limits are set on the nature and number of accessible premises. If on any given occasion we cannot predict what premises an individual can make use of (for it seems to require some form of non-demonstrative process of hypothesis formation), then, while there is always some *post hoc* account that can be made that makes sense of some result/interpretation, what is lacking is some properly *pre*dictive and so properly falsifiable account. (See W. Downes, 1984, pgs. 280 ff (and personal communication)).

[87] The quotation is a reprise of Wittgenstein's remarks we cited earlier (1953, section 131).

[88] Higginbotham reports that, at a conference in 1959, Chomsky's response, on being asked how it was that he knew that a certain string was not an English sentence, was to "argue": "What do you mean, how do I know? I am a native speaker of the English language." (J. Higginbotham, (forthcoming)).

[89] W. Levelt, (1974, pg. 6), cited in G. Adriaens, (1986, pgs. 43 - 44). Dennett makes a similar point: "Whenever we say we solved some problem 'by intuition', all that really means is that we don't know how we solved it." (D. Dennett, 1995, pg. 441). Indeed, Wright observes that: "'Intuition' suggests a primitive, unarticulated apprehension, a form of knowledge too basic and immediate to admit of any further account. ... [There is] no further story to be told ... " (C. Wright, 1989, pg. 240).

[90] W. Levelt, (1974, pg. 5).

[91] J. Bever, (1970, pg. 345). It has also been suggested (e.g. Bolinger, (1960)) that, given that judgements are meta, then one might expect the interference of normative and not necessarily very accurate notions of the sort one picks up by virtue of being educated (or indeed, by virtue of merely being a speaker of a language).

[92] N. Chomsky, (1965, pg. 27).

[93] J. A. Fodor (1985, pg. 152). The point is echoed by Higginbotham (forthcoming):

> The fact that our reflective judgements are a good source of data about our languages is a fact that calls for explanation within an overall theory of the properties of human languages and our cognitive relation to them. ... We cannot take the general reliability of reflective judgements as a brute fact, orthogonal to

> the project of exposing the structure and expressive powers of language.

The issues these observations raise are a particular problem for Platonist views of the linguistic, for if the linguistic is a Platonic realm that is ontologically other vis-à-vis the mind, then one needs some account of a) how the mind gets into any sort of relation to it, and b) some good reason why any linguistic data, mediated by minds, should be trusted at all.

[94] N. Chomsky, (1975, pg. 101).

[95] J. Higginbotham, (1987, pg. 128).

[96] Russell makes much the same point (J. Russell, 1987, pg. 228 - 229). As do Moore and Carling (1982, pg. 5): "[Chomsky] claimed that the theoretical model also explained the ability which in order to be valid it required."

[97] Not necessarily directly linguistic, where "linguistic" is defined as I-linguistic, because it is possible that intuitions do not reflect *knowledge* of language, rather they may reflect (possibly false) opinion. If intuitions are meta to the linguistic, then they can also be mistaken.

[98] The phrase is from Carroll *et al.* (1981).

[99] In P. Sells *et al.*, (1991, pg. 7).

[100] G. Harman, (1965).

[101] It is a theme that runs through *Rules and Representations*.

[102] S. Blackburn (1994, pg. 131).

[103] Examples are the hoary old favourites: "The rat the cat the dog chased ate died" (multiple centre-embedding), Bever's "The horse raced past the barn fell" (garden path) and Smith and Wilson's "This is the sort of book that, having once read it, you feel you want to give it to all your friends" (resumptive pronoun).

[104] N. Chomsky (1986, pg. 36).

[105] *Ibid.*

[106] The problem is addressed by Carroll *et al.* (1981). Their specific observation is of the unsystematicity of, and variation in, judgements. Their paper is a preliminary attempt to examine, and so ultimately specify, the "numerous other factors" of Chomsky's remark and hence explain the variation. They recognize, rightly, that the problem is not merely peripheral, but that the inability to explicate what is involved in the making of intuitional judgements is tantamount to an inability to justify the use of such data. Such an inability would amount to the "empirical apocalypse" for the generativist project to which we referred. (But let's not allow such scruples to get in the way of having stories to tell.)

[107] The phrase is from Gross (M. Gross, 1979, pg. 966). An alternative and equivalent expression is "the closed level fallacy" (G. Adriaens, 1986, pg. 48). The points raised in the present discussion are by no means novel, indeed are rather boring in their recapitulation of old concerns. And they are also

boring in the sense of being boorish: bringing standard methodology to question is rather like someone threatening to take the ball home or sitting on it so nobody gets to play. A standard way to handle bores is to ignore them and to hope they go away. But maybe, just maybe, though one hates to admit it, the reason the bore persists is because he has some perfectly legitimate concerns, and the reason why it is so much pleasanter and more convenient to ignore him is that he threatens to spoil everybody's fun. It might also be recalled from the reader's schooldays that a common justification for some, usually illicit, activity is just that it is fun so it must be alright (more or less an argument from heuristic fertility). It never seemed to work much then, did it?

[108] Our literary allusion is borrowed from Aitchison (J. Aitchison, 1996, pg. 182). Such circularity, with a concomitant unfalsifiability of theory, is what Popper inveighs against and which he takes as a symptom of pseudo-science (K. Popper, 1959). An example of such a hermeneutic circle is provided by the Freudian notion of "suppression" or of being "in denial": "Were you abused as a child?" - "Yes" (you were) - "No" (you're in denial, i.e. you were.) Compare this with the sort of story linguists concoct: is "Have you time for a drink?" grammatical? If it is not, as some analysis might predict (see footnote 112, below), then an unacceptability judgement is evidence for the analysis. If, however, such a judgement is not forthcoming, then just what plausible shuffle invoking performance factors is disallowed? Performance factors do fairly well the same job as suppression does for Freud: they constitute a carpet under which one may sweep unpalatable data. The instrument one uses being, what we termed, Occam's broom: it keeps things tidy and simple, where simplicity is defined in respect of the theory which you want to accommodate with the data.

[109] By "theory-external perspective" I am not suggesting that there are such "God's eye" perspectives that are not theory relative and external to *some* theory, but rather that there are perspectives external to *this* theory and its discourse.

[110] An example (and they are not difficult to find) is the case of the "pronouncedly deviant" "That Tom will win is likely but it's not clear which race" (Chung *et al.*, 1996). I cannot help but find this pronouncedly all right. Maybe I am wrong, but what possible sense is there to "wrong" here? A problem with intuitions is that there is little to be gained from arguing about them. Being wrong is no other than being dishonest. I am not imputing dishonesty, but what might be raised is the question of what is to be done in the case where intuitions differ and not only between individual speakers but also in the case of the same speaker having different intuitions regarding the same string on different occasions.

The standard recourse, in the first case, is to invoke the notion of the idiolect. (What this means is that you get to idealize on the lines authorized by the grammar.) Clearly this will not serve in the latter case. What we are encouraged towards is some idealization and letting the grammar decide. Unfortunately, it is not entirely clear to what extent this is distinct from giving licence for the proposer to interpret and dispose of data so as to fit the proposal: one idealizes in the direction of one's

grammar on the grounds of answering to some methodological criterion of simplicity and economy (nor is this confined to only unclear cases - see footnote 112), but what this means is that one gets to sweep unpalatabilities under the carpet, but this is only allowable on condition that simplicity is taken to be some sort of metaphysical principle that informs reality (and it is not, see below, chapter IV, 3.3) - Occam's razor is transformed magically into a broom to do the sweeping - moreover, simplicity can be defined only theory-internally so what happens is that it is one's theory that is not only the proposer as to how the world is but also the disposer. An egregious example of, in this case, letting the power of the grammar decide, is provided by Sells: "all the constructions that have been shown to require greater power than context-freeness are somewhat unusual, and apparently not run of the mill grammatical phenomena" (P. Sells, 1985, pg. 78) and so, the implication is that any untidiness (defined precisely in terms of context-freeness and the Chomsky hierarchy, i.e. the formal apparatus that is brought to the task) exhibited by natural languages need not be addressed: a case of fixing what one has to explain in relation to what one wants, or has to, explain it with.

On the other hand, in requiring that a string is taken to be determinately, irrespective of context, either in or out of the language, what is being assumed is that a principle of bivalence (a string is either grammatical or not grammatical) is relevant, that there is proper sense to the notion of a language being fixed always and already, independent of a context. In other words, that a language, as a set of sentences, is a real (in the metaphysical sense) object. This assumption is, as we suggested, brought in as part and parcel of the formal system metaphor. We aim to show, in chapter VI, that this is an unwarranted assumption.

[111] An example - again a trawl through the literature will furnish innumerable similar cases - is provided by Saito (1985). Two Japanese strings are both unacceptable (so I am informed by a Japanese speaker) with, for that speaker, no difference in their intuitive level of unacceptability. One is tagged with "?" and put down to being a performance phenomenon and the other with "*" on no evidential grounds whatsoever other than that of "letting the grammar decide", i.e. interpreting the data to fit the theory. The trouble with this strategy, to reiterate the point above, is that it is unclear what differentiates cases where it is perfectly proper (if it ever is) and cases where it is a means to getting the data to be the data that you want.

[112] An example: if you want "Have you time for a drink?" to be ungrammatical (as opposed to the grammatical "Have you got time for a drink?" (you are led to this by your analysis), then you can wave your hands and claim that the acceptability of the former is on account of it being a case of fossilized language, rather in the manner of a conventional quotational use of an archaism, somewhat in the way that certain people are given to littering their speech with Shakespearianisms or, in certain settings, usually religious, "thee" and "thou" get invoked. The judgement, or rather the hand-waving, comes out of Pollock's analysis of auxiliary verbs (Y-P. Pollock, 1989). "Have you the time?" is ungrammatical because "have" as a main verb assigns a theta-role and the only verbs, in English, that

raise over Neg/adverbs are non-theta assigning auxiliary verbs. Hence the need for an "explanation" for the acceptability of the string. It may, of course, be a correct explanation, but the worrying thing is the apparent lack of constraint on the types of factors that may be invoked in order to effect an explanation. In short, our ignorance of what goes on in performance is a licence for invention limited only by imagination and some rather vague notions of plausibility.

[113] These terms (borrowed from Dennett (1987) and he in turn from Reichenbach), are roughly equivalent to *representatum* and *representans* respectively. Some more flesh will be provided below (chapter II, 4.0).

[114] C. Hutton (1993, pg. 177).

[115] This is a quite explicit foundational assumption of the generativist paradigm: Chomsky, in beginning the third chapter of *Syntactic Structures*, remarks: "Assuming the set of grammatical sentences of English to be given, we now ask what sort of device can produce this set." (N. Chomsky, (1957, pg. 18)). The device being, in more recent parlance, the I-language.

[116] Bivalence is a standard accompaniment of metaphysical realism, i.e. the thesis that the facts of the matter are a determinate totality, fixed once and for all whether or not these facts come to be experienced, or, indeed, can be experienced. This is the sense of "transcendent" in the above paragraph: what is the case is independent of its being recognized as the case. The facts of the matter are always and already. See, for example, H. Putnam, (1989, pg. 214) and A. Grayling (1990, pg. 233).

[117] Recall our remark (paraphrasing Smith) that if some account allows itself to unquestioningly rely on some categorization, then it is not too much of a stretch to imagine that the results are going to be skewed in that categorization's favour. We might say skeuomorphed in that categorization's favour.

An alternative name is "brain puns" (J. Cohen and I. Stewart, 1994, pgs. 22 - 24). The idea is closely related to our previous discussion of the role of metaphor in scientific hypotheses and our observation of a Woozle hunt: what we see is what we put there; skeuomorphs are the trophies of Woozle hunts. (Also compare our quotation (pg. 26) from Smith.)

[118] N. Chomsky (1991, pg. 50). We are a little guilty here of over-extending the reference of "conclusions", Chomsky has in mind the particular conclusions of the Minimalist program. However, we will be concerned with showing that these latter conclusions are, indeed, the result of taking to its limits the style of theory construction we have been concerned with identifying.

CHAPTER II.

Instrumentalism versus Realism.

How exquisitely the individual mind to the external world
Is fitted - and how exquisitely, too
The external world is fitted to the mind
And the Creation (by no lower name
Can it be called) which they with blended might
Accomplish.
(W. Wordsworth)

I was myself the compass of that sea:
I was the world in which I walked, and what I saw
Or heard or felt came not but from myself;
(W. Stevens)

1.0 Introduction.

Our earlier discussion remarked on two species of approach to the use of a formal or conceptual system to model some phenomenon. The former, the Galilean, has it that such a system is able to model some given phenomenon because that which is being modelled inherently corresponds to the properties of the formal system, as if it were the instantiation by some other means of that logically prior formal system, a sort of writing in some other medium which translates entirely from the language of the formal system without residue. The Einsteinian observation, on the other hand (see pg. 30), makes recognizable a distinction that is available to be drawn: the formal system is a vehicle that allows one to talk about some object of study, to make correct predictions regarding that object, but that object in itself exceeds, i.e. is logically distinct from and autonomous of, the properties of the formal system *qua* formal system. The former construal is resolutely realist and Galilean regarding the properties of formal and conceptual systems: such give a transparent picture of an independently existing world, because that world is prefigured by some such system. The latter approach, in recognizing a proper distinction between *representans* and

representatum, confers a degree of opacity on the vehicle which articulates theory. When this opacity is taken as ineliminable the result is an instrumentalist view of scientific enquiry. This view has it that there is no literal truth or falsity possible to scientific theories: scientific knowledge is not of an independent world but is only the projection of mind-dependent and mind-projected concepts:

> The study of a (putatively) "empirical" domain is not to be viewed as a "direct" study of the domain itself, but rather a study of our knowledge of it.[1r]

This view is closely related to the Quinean observation of the theory dependence of any and all observational statements[2r] and the correlative criteria for success of any theory: it is not a case of a theory being successful by virtue of its correspondence to facts of the matter in the world, but rather a theory is successful when it is consistent with a web of beliefs viewed holistically. A corollary is the possible revisability of all beliefs, because those beliefs are not guaranteed by any real-world, mind-independent facts of the matter. The revisability is claimed to be relative to their coherence with all others in our conceptual scheme. In brief, what one gets is a relativism.

One step further leads one towards an out and out idealism: if the facts of the world are knowable not as facts of the world but only as projections of our conceptual scheme, then the world is indistinguishable from a fiction woven out of just that web of concepts: "To put the conclusion crudely, the stuff of the world is mind-stuff."[3r]

2.0 Reasons for realism.

To avoid a philosophical morass, we will take the line, as more of an assumption, even pretence, rather than as an unassailable position, that, regardless of the philosophical plausibility of a thorough-going and ineliminable instrumentalism, scientific enquiry more or less consistently assumes, in practice, a realist perspective and to revise that assumption would be at the cost of the prime motivating factor in scientific enquiry: it is founded on an implicit metaphysical assumption that there are some mind-/concept-external and independent states of affairs which it is the task of science to elucidate. Furthermore, the development of the sciences is most accessibly understood in terms of the preference for one theory over another on the basis of a theory's answerability to the way the world in fact is. The fundamental problem with

relativism is that it needs to account for the fact that, whenever enquiry is serious, whatever field we are concerned with, it is not Liberty Hall:[4] there are certain things that simply cannot cogently be said of some phenomenon. It is reasonable to suppose that what makes things impossible to be said is the way the world in fact is, and that it is the way the world in fact is that is, potentially, the ultimate arbiter of theory. To put this another way, if the sciences are, in fact, in the business of concocting stories to spin, if they are a species of fiction, then not only has science misunderstood itself (and we come to fail to understand what has been taken for its success), but also it rather loses its purpose, not to mention its constraints: there seems little motivation to "advance" science by merely exchanging one myth for not only some other, but *any* other: everyone might as well go home.

A riposte might be to remark on the motivation offered by the preferability of a set of beliefs that is internally coherent and it is the achievement of such coherence, as opposed to correspondence to states of affairs, which constitutes an "advance". A coherence theory of truth is to take the place of a theory of truth as correspondence to external world facts. However, this is merely to exchange one metaphysical assumption for another: while realism makes an assumption of an external world to which theories are answerable, which assumption the philosophers show us is difficult to make good, the coherence view (e.g. Quine, (1953)), which we will take as symptomatic of positions that are sceptical of a scientific realism, has a similar difficulty.

The Quinean slogan has it that all beliefs are revisable. This raises something of a paradox because if we take it at face value, then the belief that "all beliefs are revisable" must also be revisable, so it cannot be true, the only revision available is "not all beliefs are revisable." One might try to sidestep this, Wittgenstein-like, by claiming some meta-level perspective, some ladder which is to be thrown away after use. However, the point of the original slogan seems to depend on the possibility of something true (in the realist and not the pragmatist sense of the term) being able to be said of something, and this is to make a realist assumption. In this case, what constitutes the determinate and real something about which something true can be said is not some external world but a web of beliefs. The particular problem arises when one asks how we are to revise that web: what is it that makes one theory preferable to another? The answer is, as canvassed, a theory's coherence with the rest of the web of beliefs. But, in this case, there needs to be an unrevisable, prior and real criterion of coherence: "Being presupposed in the very concept of a system, [a principle of coherence is] at least absolutely *a priori*."[5r] Without such there are, literally, no

coherent means, and so no coherent reason, to prefer one theory over another. In other words, a similar metaphysical and realist assumption to that made by realists is in place except in this case it is not an assumption of an external set of facts of the matter, rather it is of an external/meta-level principle relating beliefs to each other. It seems one cannot avoid some meta-(physical) assumption; either that the external world is that to which theories are answerable, or that there is some *a priori* notion of coherence, or, perhaps, both.

One is also drawn to wonder whence comes this metaphysical obligation to have a set of coherent beliefs: as the poet (Whitman) points out:

> Do I contradict myself?
> Very well then I contradict myself,
> (I am large, I contain multitudes).

More prosaically, it appears to be the case that, for instance, many people have mutually inconsistent ways of understanding electricity - as fluid or as a bundle of particles - each manner of conceptualization being successful in respect of certain problems but not others where the rival conceptualization works (also compare wave-particle duality in quantum mechanics[6]). Why, and what use is there for a requirement to make these conceptualizations consistent, unless one has in mind the realist assumption that there is one and only one correct theory, or at least that there is potential content to the notion of best theory, where best is in respect of the fit between theory and how the world happens to be?

In the light of these remarks, we will assume that a scientific realism (construed as the thesis that the way the world is is what, to some crucial extent, interprets theory and is what theories are answerable to,) is a viable option, for it seems not to go away. We will also assume - an assumption that will be implicit in our methodology - that the standard notions of truth and falsity (i.e. a statement is true iff it corresponds to the facts of the matter) can be, in principle, properly, at least virtually, predicated of scientific theories.

The qualifications in this statement of what we are assuming viable are for the purpose of distancing our position from an unqualified realism. To recap, such a metaphysical, "God's Eye point of view" realism holds that facts of the matter are independent of minds and that "the world consists of some fixed totality of mind-independent objects"[7r], these objects having their properties inherently. What we have been insisting on, even if implicitly, is a species of realism along the lines of Putnam's "internal realism" and which takes note that:

> our way of understanding the world in terms of objects, properties and relations is an imposition of our conceptual schemes upon external reality; reality as we understand it [and as we can only understand it, its the only access there is] is structured by our conceptual schemes.[8r]

From this perspective, facts of the matter, objects and their properties, are not so much always and already, inherent and waiting to be discovered, rather "'objects' [and their properties] do not exist independently of conceptual schemes."[9r] This does not amount to an idealism, but it does amount to a recognition that our epistemic access is always and only by way of the concepts that are available to us. However, and here is the point, while constructs and properties are what get put there as initially skeuomorphs, a sort of virtual reality and "map of the territory", the present contention - and which provides content to the realism and what might excise the metaphoricity - is that the crucial arbiter of whether they can be properly put there is the world itself; whether it accepts them in the sense of conforming to, not behaving other than, this, as it were, virtual reality. As Bromberger puts it: "surveyors label and map the territory, ... but they don't create it"[10r] and, of course, what decides whether a map is more or less accurate is our trying to use it in respect of negotiating our way through the territory itself.

In the light of this, and to be borne in mind in our discussion, "facts of the matter" are to be reconstrued as the external world's acceptance of conferred properties; theory development and the scientific enterprise are rather like trying out conceptual clothing on the world until there is a maximal fit; it is undertaken in the belief, or faith, possibly mistaken, that a perfect fit can be achieved. Taking this line we get to maintain the standard realist view that there is content to the need to choose between theories in respect of which is best "accepted" by the objects under study, which fits the most closely: it is the external world that remains the crucial arbiter of a theory's correctness.

It is, however, one thing to argue for this position, it is another to take the same argument, even if it were conclusive, to found the assumption that a theory, in so far as it is confirmed by its predictions, might make such successful predictions *only* on the basis of its correspondence to (or, in the light of our remarks, best acceptance by) the way the world in fact is; that successful predictions are in themselves sufficient to warrant the constructs of a theory as (virtually) real.

3.0 Reasons for an instrumentalist perspective.

There is a persistent temptation in respect of opposing and polarized philosophical positions, a temptation founded on, perhaps, a desire for tidiness, to see opposing poles as a case of either/or (in a strictly exclusive sense). A case in point is Carr's discussion of this issue.[11r] It seems that either one is to be a realist or an instrumentalist; what he appears to neglect is the possibility that certain theories warrant a realist, but others no more than an instrumentalist, construal. Carr apparently takes the instrumentalist/realist debate to be a case of an exclusive disjunction: in finding against instrumentalism, then any theories that make successful predictions are to be taken as, *ipso facto*, referring to real states of affairs, as if the only basis upon which a successful prediction could be made were one of the reality of the constructs that enable the truth of the prediction.

Carr argues for a realist construal of linguistic theories on perfectly cogent and well-authorized grounds. However, what he argues for is the general cogency of realism in the philosophy of science, and hence the possibility of some more or less finished linguistic theory that does correspond to the real facts of the matter, facts which are guaranteed by the way the object happens to be. This is to be distinguished from an argument that shows that any given, actual linguistic theory is to be properly construed as corresponding to, or to be about, such facts: because there is, putatively, a discrete phenomenon to be explained does not entail that a theory that calls itself, in this case, linguistic, is, *ipso facto*, making reference to the real linguistic facts of the matter. What Carr requires for his argument to get started is some case to be made for our having confidence that the object of study as postulated by current linguistic theories is properly identified, that our initial carving of the world has selected an object that is determinate and determinable and about which one may have a realist theory at all. Raising this returns us to the question of the originary choice of an object of study, a choice, we have intimated, that was selected for by the availability of certain formal systems.

Carr's confidence that a proper object has been identified, and that linguistic theories are disconfirmable in respect of that proper object of study, is based on the contention that:

> there is no way of accounting for the success of our theories other than by adopting [a] realist position, the principal warrant for which is heuristic fertility.[12r]

By this phrase he intends a theory's ability to make successful predictions and to uncover previously unrecognized phenomena. (One might mention that one other entirely feasible way of accounting for this "success" is just the "closed circuit functioning" we identified - compare the "success" and heuristic fertility of Freudian psychology.) Leaving these remarks in parentheses, Carr points out that it is in respect of such an ability that one theory might be preferable to some other less good theory. Moreover, the fact that theories can be compared in this regard itself argues for a realism about scientific theories: what grounds the "heuristic fertility" of a better theory is that theory's closer approximation to the facts of the matter in the world: it is the world that is the test and theories stand or fall in relation to their ability to correspond to the way the world is.

Chomsky's position is not dissimilar to Carr's, the discernible difference being, as Carr points out,[13r] Chomsky's summary dismissal of instrumentalism as somehow obviously discredited, as a view that was "rapidly discarded". In his arguing that linguistics be treated in the same manner as the physical sciences there appears to be an assumption that realism is the universally accepted view.[14] Chomsky argues[15r] that proposed linguistic theories should face the same criteria, neither more nor less stringent, as those which the theories of the other natural sciences face. Chomsky points out that it is "social practice" to confer on best theories the status of "true" and on their constructs the status of "real", much in the manner that we are happy to include the hypothesized particles of sub-atomic physics in our general ontology. In the same way, to the extent that a linguistic theory makes successful predictions, then, so the rhetorical question goes, what further criteria are to be met over and above predicting the data?

While it may well be social practice to confer truth on best theories, social practice is not, I understand, the arbiter of whether or not something is true, at least not unless we subscribe to some species of pragmatism.[16] Where Chomsky's argument does have force is in respect of predictive success. Such success, goes the Chomskyan argument, warrants the propriety of the predicate "true", and "true" here is not to be distinguished from "psychologically real": a linguist's grammar provides, by virtue of its postulated mental representation, a causal explanation of the nature of natural language, of its acquisition, of the means to linguistic creativity and of the similarities between natural languages.

It is at this point that Chomsky's and Carr's positions bifurcate. Their agreement is over the propriety of a realist construal of linguistic theories that achieve heuristic fertility, their disagreement is over the ontological status of linguistic objects:

Chomsky takes a psychological stance, Carr views the linguistic as constituting an autonomous domain that cannot be reduced to the psychological. In terms of our initial schema, the disagreement is over the move from the second strand of Chomsky's revolution to the third, from realism to psychological realism.

While we will defer detailed discussion of these issues, it will, however, be useful, as a means to orientate ourselves, to baldly state what we hope to substantiate.

Autonomy of a domain involves the claim that that domain cannot be contentfully related to any other. Arguably, what sustains the willingness to concede the psychologism of Chomskyan linguistic theories is the (possibly mistaken) obviousness of the relation between the linguistic and the psychological (a lot hangs on what exactly constitutes "the psychological", but we will skip over the problem for the moment), for how else might one conceive of explaining why natural languages are as they are other than by citing the human mind/brain. If one wants an explanatory linguistics - an answer as to why as well as what, then, seemingly, one needs a psychological linguistics. A theoretical position that results in the inability to relate the linguistic to the psychological constitutes, on the basis of this obviousness and this explanatory need, grounds for questioning that theoretical position: the autonomy of the linguistic is tantamount to the anomaly of the linguistic, and the inability or refusal to explain the linguistic. If Chomsky's position cannot be adequately substantiated such that it achieves some identifiable psychological content, in other words if Carr (and Platonists such as Katz *et al.*) are correct, then this leaves us having to negotiate an anomaly. One way to so negotiate the anomaly is to drop a realist perspective and exchange it for an instrumentalist position.

Carr's and Chomsky's realist construal of theories depends on their arguments from heuristic fertility. Instrumentalism has it that there are always insufficient grounds for confidence that the world corresponds to the constructs of a scientific theory: it is not, until it becomes an idealism, a scepticism about the real world, but a scepticism of our knowledge of it. Our discussion (somewhat summarily) reinstituted the cogency of such a realism. The question is: when are we warranted in construing a theory's constructs as corresponding to the real state of things? It cannot be the case that the possibility of a realism thereby confers reality on the constructs of even "best theories" (unless we take Chomsky's Peircean suggestion and take social practice to be the arbiter). This is for the simple reason that best theories have an uncanny habit of becoming second best, *passé* good tries, in other words, not true. Presumably,

defunct best theories achieved their "best" status by virtue of the heuristic fertility and predictive success that Chomsky and Carr advert to. But how are we to think of these partially truth-bearing, second-best theories? One answer is as instrumental fictions. The point is that bald, unqualified and unsupplemented heuristic fertility cannot be a sufficient mark that a theory answers to the real facts of the matter, as if predictive success could only be achieved on the basis of a theory's constructs corresponding to real entities of the real world.

Before adding some flesh to these remarks, it is worth pointing out why the possibility that the most appropriate construal of competence linguistic theories and their objects is as instrumental fictions might have a certain *prima facie* appeal. In short, the appeal is that it appears to make sense of some of the points that we have touched on.

One motivation for taking an instrumentalist perspective on theories in Chomsky's generativist tradition is that it avoids the ontological bifurcation that would be motivated if it were shown, as we intend, that the psychologism of the Chomskyan project is unachieved. If no psychological ontological status can be granted to linguistic objects, then, if one is to persist in a realism towards those objects, it is necessary to grant them some ontological status. The result of an autonomism would be to add one more different sort of thing to our ontological catalogue. Such might be the case, but, given the prejudices of a scientific materialism, and its fondness for Occam's razor and a clean-shaved chin, it is an unpalatable move. On the other hand, if a psychological linguistics is, given the basic prejudices of the cognitivist paradigm, what we want, and that is what we don't have, then an instrumentalism offers a way to avoid those prejudices being embarrassed: one does not usefully quantify over fictional entities, and nor can one conclude from such entities to some non-psychological status for the linguistic.

Secondly, an instrumentalism offers a perspective on the problem we outlined earlier, that of the difficulty of adducing sufficient grounds to allow for decision between competing frameworks. As remarked, the present state of the field shows a degree of fracture, at least of pluralization, while, for the most part maintaining an agreed object of study: a competence grammar. Each framework continues as a going concern by virtue of precisely the "heuristic fertility" Carr adverts to. If such fertility were sufficient warrant for each framework/theory to be construed as corresponding to the real facts of the matter, i.e. to be construed realistically, then linguistic reality becomes worryingly pluralized. A question this raises is that, if we assume that at most

one current framework is true, then how are we to account for the comparable heuristic fertility of those which are not true?

Returning to the insufficiency of Carr's (and Chomsky's) argument from heuristic fertility for the grounding of a realist construal of theory: the point we sketched regarding superseded "best theories" is an instance of a standard argument for instrumentalism that goes by the sonorous name of the pessimistic meta-induction.[17r] The argument is remarkably simple but powerful for all that: the history of science is littered with defunct, because falsified or superseded, best theories. On inductive principles we can surmise that present best theories will also turn out to be disconfirmed. On these grounds we would be unwise to confer truth on any current theories or reality on their constructs.

This argument, as well as encouraging a thoroughgoing instrumentalism, also raises something of a paradox. If, sanguinely, we take a Whiggish view and think of science as a progression that, in the long run, gets ever closer to the truth, then the nearer one gets to the finished theory the more inductive grounds (because more superseded theories) there are from which to conclude the falsity of any current theory. Paradox aside, it would, however, as Papineau points out, "be too quick to conclude that [the argument] discredits realism completely."[18r] He remarks that:

> the tendency to falsity is much more common in some areas of science than others, [there is] a differential success rate [which is] the result of the necessary evidence being more easily available in some areas of science than in others.[19r]

The moral he draws is that "we should be instrumentalist about that sub-class of theories which are not supported by adequate evidence."[20] Arguably, we should be similarly inclined about those theories that furnish their evidence out of some "closed-circuit functioning", and also so inclined about those theories that *cannot*, in practice, adduce either evidence nor adequate criteria to substantiate their claims such as would allow for decision between competing accounts.

A useful illustration of an instance of a predictively successful, "heuristically fertile", best theory that cannot be construed realistically is the Ptolemaic theory of the solar system. Given all the data that were available to the ancients (i.e. naked eye observations) the Ptolemaic system was outstandingly successful: it, as it were,

achieved coverage. Predictions of planetary motion were made on the basis of a geo-centric model with the Earth (and human-kind) resolutely in the centre of things and the celestial bodies in steady progression around that centre. The idea of a static Earth is, of course, not only eminently preferable (out of species-egoism and in keeping with both scripture and a Christian world-view), but also eminently obvious: it is the sun and the moon and stars that move, one only has to look. Given all the available data, the available conceptual scheme and the success of the theory, there was simply no motivation to consider any alternative.[21]

Complications in the Ptolemaic geo-centric scheme were induced by the fact that the planets do not conform to movements that are consistent with simple circular orbits about the Earth. To get round this, (after all, why drop the basic assumption that allows one to explain so much, is this not what we do in linguistics?) it was necessary to have the planets moving in epicycles, i.e. minor orbits having as their foci the primary orbits around the Earth. (Imagine a large wheel with a smaller wheel having as its axis some point on the circumference of the larger.) Given this adjustment then you get all the data out. To be noted here is a variation on the theme of a skeuomorph: an unquestioned, perhaps a mediaevally self-evident, assumption is that celestial motions are to be explained (theories are to be posited) in terms of circular orbits. (The grounds being the perfection of this geometrical figure, the heavens being the creation of a, by definition, perfect divinity, and hence, the heavens themselves must be perfect. *Ergo*, celestial motion involves circles.[22]) Circles are, as a result of these considerations, the conceptual apparatus and explanatory construct to which one is constrained in positing theories: theorizing is "limited by the materials available".

What ultimately upset this marvellously successful theory - a theory that was rather more predictively accurate than Copernicus' original counter-hypothesis - were some extra and unfortunate data from telescopic observations (Galileo's) that showed that, amongst other things, Venus, like the Moon, has phases. Given these data and a willingness to commit heresy, the conclusion, or rather, the hypothesis, is that it is the Earth that moves. In hindsight it appears somewhat comical that a considerable debate of the period centred on the admissibility of telescopic data, indeed on the admissibility of any data other than naked eye observation, as if the data were somehow *a priori*. It is worth pointing out that Fodor charges Katz with just such *a priorism* in attempting to stipulate what linguistic data are.[23r] It is also worth mentioning in passing how this "comical" debate bears an uncanny resemblance to that regarding the pertinence of psycholinguistic evidence in the disconfirmation of

linguistic theory.[24] However, the point to be underlined here is that while the Ptolemaic theory is effectively disconfirmed, there remains the problematic status of the true predictions that were made on the basis of this false theory. Clearly, it cannot be the case that the theory's "heuristic fertility" is to be taken as warrant to construe the constructs of the theory (geo-centricism and planetary epicycles) as answering to the real facts of the matter. Rather, to the extent that the theory is truth-bearing, then these constructs of the theory are to be regarded as fictions that are no more than instrumental in providing predictive success: it is "as if" the Ptolemaic model pertained.

4.0 Warrants for realism.

> *Other sciences are concerned with objects that are given in advance and which one can consider from different points of view; in our field there is nothing similar.* (Saussure)[23]

While we may agree that there is no present warrant to construe a Ptolemaic account realistically, we may ask what it is about the theory that has superseded it that encourages a realism. This returns us to the question of what Papineau might intend by "adequate evidence" such as would demarcate theories meriting a realist construal from those that merit an instrumentalist interpretation. In raising this we are also addressing the question of how our charge of "closed-circuit functioning" in theory development might be negotiated; how one might be persuaded of the excision of a skeuomorph effect and, correlatively, of the excision of the fictionality/metaphoricity of the theoretical constructs posited: in brief, how to get out of the closed circuit.

In taking up this issue we are also setting ourselves an agenda and an orientation for our discussion of Chomsky's psychological realism. For now we will state rather baldly what we see as requirements and leave the details to emerge in our further treatment.

A pre-condition on a realist construal of some theory is that it state clearly what entities and relations it takes to be real; that is, there is a requirement that a theory specify what it postulates to exist as, in Chomsky's phrase, "real objects of the real world." In the case of deductively formulated theory, this is a requirement to be met by a suitably explicit interpreting function mapping from terms onto entities (and

sets of entities) in a model. In meeting this requirement it is also desirable, and arguably a corollary, that these objects are provided with some statement of their ontological status: we need to be told what it is we are supposed to be talking about and what sort of things they are.[26]

The general point is that only by providing such specifications are we likely to be able to adduce and apply adequate criteria of identity for the constructs of some theory. One can't usefully apply any such criteria if we are not told what they are to be applied to. Furthermore, we need such criteria of identity if we are to take seriously the task of deciding between theories, i.e. grammars, and if we are to credit existence to any particular set of postulated constructs; the meeting of such criteria will be a condition on our acceding to their existence.

Once such preconditions are satisfied (in effect, no more than meeting the need to be clear in what is being claimed, i.e. stating which theory is being articulated through the vehicle of, in this case, some formal system), then, in order to have some confidence in the reality of the constructs postulated (e.g. the modularization of the gross phenomenon and the entities interpreting the terms and relations of the calculus), those constructs need to be able to be corroborated in such a way as to avoid the theory-internality we have described.

To reiterate: confidence in the appropriacy of a realist construal is not warranted on the basis merely of some calculus' ability to achieve coverage. This on two counts: firstly, on the grounds that there is a need to choose between grammars that have equivalent coverage - at most one can be right; and, secondly, we saw that, in the case of the "heuristically fertile" Ptolemaic system, what enabled the predictive success was, precisely, fictional constructs, i.e. the geo-centric model with its correlative, saving-of-appearances, epicycles.[27] What is needed is some additional correspondence principle beyond that of achieving coverage.[28]

The point of the demand for an escape from the theory-internality we identified is that, in order for us to be warranted in conferring existence on any posited constructs, we need to have grounds for confidence that those constructs have not simply been put there as a function of (an impulse to a referential theory of meaning for the syntactic terms of) the calculus that provides us with some initial, theory-constitutive epistemic access: "notation systems create analyses", and different notations create different analyses and correlatively create different sets of entities that interpret the terms and relations of those analyses. At best, if there are facts of the matter, all but one of these sets must be fictional. Given that such fictionality is the

case, then the point of asking for some non-theory-internal, non-closed-circuit perspective, (some "other point of view",) is that, presumably, fictional entities leave no traces and are indiscernible beyond constituting objects that are projected so as to interpret the terms of each calculus: for example, there is no hope of ever adducing evidence for the existence of planetary epicycles, *outside* of the limited perspective of a restricted and stipulated set of data, by observation by some inter-planetary probe, for example, precisely because they are fictional. Epicycles are only "observed" as theoretical constructs when hypothesized as the case in order to account for a restricted range of data and when hypothesized on the basis of certain initial assumptions. These assumptions amount to expectations of what the world is (and ought to be) like: geo-centric and reflecting a creator who has a penchant for geometrically "perfect" figures. Indeed, they may be taken as paradigmatic of skeuomorphs, reflecting the imposition of some conceptual scheme and ordering and observing the world such that it conforms to, and is conflated with, that scheme.

A homely illustration is in order:[29] A calculus or formal system is used to articulate or, let us say, picture some object or set of phenomena. Consider a painting (realist style) that claims to be an accurate, hence true, representation of, let's say, a landscape. One might point to the picture and ask someone what it is. There are two perfectly good answers: "It's a landscape" or "It's a picture (of a landscape)". The former reply takes the medium as transparent, which is to say does not distinguish, as the latter does, between the *representans* and the *representatum*,[30] moreover, it idly confers some existence on the landscape *qua* landscape, rather than conferring existence on the picture *qua* marks of paint on canvas as the vehicle of representation. Of course, in the ordinary course of things this is of no great import. However, if we are interested, the only way to ascertain, if asked, whether it is an accurate and non-fictional picture or not, is from the perspective of the latter reply. Crucially, and a point to be taken up below, answering this question also requires the availability of the actual landscape depicted; the object needs to be discerned independently of its representation by the picture; in other words, it needs to be discerned from a different point of view, through other spectacles, otherwise there are no grounds for confidence that the object is any more than the creation of the artist's painterly technique. When this technique is supplemented by an observer's "natural impulse" to a metaphysical realism, then, in the "it's a landscape" answer to our question, what arises is an illicit ontological commitment to constructs that would interpret the terms/technique of the artist.

To place these comments in relation to the role of the formal apparatus of linguistic theorizing, in brief, the requirement is that "the theory that accompanies the notation must tell us which aspects are intended to be empirically significant."[31] This is to be stated through the interpreting function that defines the modelling relation between the formal system and the object being modelled. Implicit in this remark is the recognition that certain aspects of the formal apparatus need not be interpreted as referring to any entities or properties of the object modelled. Such aspects remain resolutely properties of the vehicle of representation, having a purely instrumental role: what we get is a distinction between *representans* and *representatum*, between, as it were, the painterly technique and the representational task to which it is applied.

To illustrate, consider again our picture metaphor. Let us suppose that a painting is an accurate representation of a real landscape. Looking at the picture we can discern objects that correspond to objects in the real world. Thinking of the picture as a hypothesis of what the world is like we would take it that the artist intends us to quantify over the objects as depicted: there are, really, those two trees and the hedgerow and the gate. If we look closer we can discern the individual brushstrokes and the manner in which the paint has been smeared to create certain effects. At this level of discrimination we would not be tempted to the view that these elements of the painter's style are intended to correspond to real-world entities; these elements are purely instrumental to the depiction and it would be a mistake to quantify over them on the basis of the fact that they are the means to achieve an accurate account.

Less informally, and turning to the role of mathematical models in the sciences, Matthews supplies an illustration (from physics) of the distinction we are suggesting: "In the case of Bulmer formulae for the spectral lines of hydrogen ... the integers of that formula initially received no physical interpretation."[32r] What underlies these remarks is the recognition that the purely instrumental/representing aspect of the calculus is to be differentiated from those elements which are interpreted to refer to the real linguistic objects to which the theory is existentially committed.

The distinction being made here rehearses points that have run through our discussion. In Matthews' terminology the distinction is between "representational constructs" in contradistinction to "theoretical magnitudes."[33r] The former refer to aspects of the vehicle, the conceptual apparatus, which articulates theory and which aspects remain resolutely unquantified over in the world, whereas the latter refer to those aspects/terms of the vehicle that are taken to correspond to real world entities. The point is best made by way of Matthews' illustration (borrowed from Ryle[34r]) of a theory of an individual's terrestrial location. One may specify such a location in terms

of a mathematically articulated apparatus of latitude and longitude. The model provided allows one to pick out any location, e.g. that place where I am typing this, in Matthews' terms a particular "theoretical magnitude". It is, presumably, something one would be willing to quantify over as having a concept/mind-independent and concrete existence: it even fulfils Dr. Johnson's rather stringent criterion for existence by way of being something that one can kick. However, while one can kick this location, one cannot kick the point that is the intersection of a line of longitude with a line of latitude, for that is a different order of thing; an abstract, extensionless entity which subsists as a projection of the conceptual apparatus which articulates a theory of terrestrial location, and which has no existence independent of this conceptual apparatus - in short, it is a "representational construct". I suppose, one may, if one really wants to, or has to, get all platonic about these mathematical entities and grant them a mind-independent existence. But this does not entail that there are objects or phenomena in the non-platonic world that they ineluctably model, although there will always be *fictional* objects and phenomena which are the model of such mathematical structures.[35] This latter point is the crux of my misgivings under the heading of a skeuomorph effect: given the nature of the data, there are insufficient means to determine whether or not the fictionality of these always and already objects, brought in as projections of the conceptual apparatus, can be excised.

This distinction between theoretical magnitudes and representational constructs corresponds to that posited by Ryle and Dennett[36] (and, reportedly, Reichenbach[37r]) between *abstracta* (correlating with representational constructs) and *illata* (theoretical magnitudes). In essence, recognition of the distinction adverts to the need to be aware of the role of the conceptual glasses of Wittgenstein's metaphor in positing theoretical constructs and the need to distinguish between what is apparatus/*abstractum* and what is quantified over as independently real and inherent to the object of study/*illatum*. This translates into a requirement on a theory that it specify (and is able to (dis)-confirm) what terms and relations of the formal theoretical apparatus are claimed to exist as, i.e. be interpreted by objects that are, theory-externally real, and which exist (and can be (dis)-confirmed) independently of the formal apparatus that prefigures that reality.[38]

This requirement bears directly on the issue of the pluralization of grammatical frameworks in linguistics, which pluralization, to some, might look like gratuitous duplication and gratuitous because there does not appear any prospect of closure: one might reasonably ask what is the point/content and, what amounts to the same, what is the difference and what is the point/content of the difference between competing

accounts. The problem is one of how such analyses, with their correlative constructs, get to be disconfirmed, and this itself refers us back to the problem of closed-circuit functioning which we identified. To put this another way, if two extensionally equivalent frameworks are taken to articulate different theories by virtue of making putatively distinct empirical claims (they quantify over different and non-logically equivalent sets of linguistic objects), then this has content just in case, firstly, that there is some clear specification of theoretical commitments, i.e. what is quantified over, and, secondly, that there are some potentially available and appropriate means to decide between those claims such that one may vitiate any charge of theory-internal circularity in theory confirmation.[39]

In illustration, for now no more than a suggestive sketch, of how this latter might be achieved it will be useful to return to our picture metaphor: one might say that it provides us with a certain epistemic access onto the issue. If we conceive of our landscape as a theory about some aspect of the world, then what we need to give us confidence that what is depicted corresponds to some state of affairs having its properties independently of its depiction is some alternative access to that putative state of affairs; an access that is not dependent on the initial, theory-constitutive depiction. In brief, what we need are other means of access or, to conjoin Saussure's and Wittgenstein's metaphors, some other points of view, some different spectacles. If the same things turn up irrespective of which spectacles we are wearing, then, it would seem, that what we get to see is not simply a function of any one and particular pair of spectacles, i.e. any one means to gain some epistemic access, rather we would seem to be justified in concluding that what is presented to us reflects some independent state of affairs in the real world. It is just such varied corroboration, by different means of observation and measurement, or by the commensurability of the constructs of different theoretical perspectives, that might constitute warrant for a realist construal of theoretical constructs.

For informal example, consider the notion of an electron: whether we are coming from the perspective of physics or of chemistry it turns out that neither field can do without just these posited entities. Or, to put this another way, the entities that get to be posited from the physicist's perspective turn out to be commensurable and able to be identified with the entities that are posited from the chemist's perspective. A similar mutual, but independent, corroboration is what underlies confidence in a heliocentric model of the solar system: Kepler's more or less observationally accurate mathematical laws and the sun-centred model that interprets them fall out as a consequence of a theory of gravitation. In both cases, the two fields of enquiry act in

a confirmatory dialectic, indeed, in just the way that is denied to grammatical theories by reason of the availability of some plausible shuffle that gets to have an appearance of being half-principled by way of pointing to the unspecified yet "numerous other factors" that might contaminate otherwise disconfirming data. What this supposedly licensed shuffle achieves is the unfalsifiability we noted and at the same time (and this because of) the maintenance of the closed-circuit functioning. On the other hand, what the availability and pertinence of data from other perspectives would permit is the ability to step outside this very circularity. Furthermore, it is for this reason that the ontological status of theoretical constructs is of some immediate importance. To state the case baldly: if such constructs are claimed to be psychological (whatever that might mean exactly), then one could expect that the constructs of linguistic theories might be observable from the perspectives of theories in adjacent, psychological fields. On the other hand, the only seriously considered ontological alternative - some form of Platonism or autonomism - would appear to make, at best, problematic any such commensurability between linguistic theories and theories in other domains, leaving us, in this regard, empirically stranded. We take up these issues in the next chapter.

5.0 Summary.

At this point it will be useful to summarize the position we have reached and to indicate the direction we will be going. Centrally, we have argued for the possibility of an ultimately realist construal of scientific theories. In doing so we have pointed out that this is not (*contra* Carr) equivalent to denying the viability or the preferability of an instrumentalist interpretation in respect of theories in certain fields of enquiry. The central point we raised in support of this view is that the ability of a theory to be truth-bearing, i.e. to enable correct predictions, need not be on the basis of the correspondence of the constructs hypothesized to the theory-external facts of the matter. Rather, we have suggested possible criteria to be met such as would provide warrant for a realist construal. While these particular criteria are negotiable[40] (i.e. specification of ontological commitments and attainment of some vantage point that does not assume and is independent of the theory that posits and observes its own terms and constructs), what is not negotiable is the general requirement that some means be adduced for distinguishing between the claims of competing accounts, where those competing accounts claim to capture determinate facts of the matter. The

meeting of such a requirement is a condition on the annulling of the suspicions we raised under the heading of skeuomorph effects: that rather than it being some putative facts that are being answered to, instead these 'facts' are confused with, and indistinguishable from, fictions that are projected so as to interpret the terms and relations that are brought in by way of the formal apparatus applied to in the task of theory construction.

It will be the purpose of the following chapter to show that Chomsky's psychological realism - the manner in which a determinate set of facts of the matter gets to be predicated of the linguistic - fails to achieve proper content and collapses into, at best, an autonomism/Platonism. This is itself, we will argue, an unpalatable position for it leaves unanswered the question of the relation between individuals and the languages of which they are speakers: we are denied a fully explanatory linguistics. Furthermore, if such a position can itself be shown to warrant no more than an instrumentalist construal (which would, as a by product, at least relieve us from the corresponding ontological commitments), then this would lead us to enquire what it is that obstructs a realism towards linguistic theories.

In substantiating the appropriateness of an instrumentalist construal, part of our task will be to show that the preconditions and warrants for a realism that we have sketched are systematically left unsatisfied. Our focus will then be on the criteria, some additional correspondence principles, that have been proposed, either explicitly (Chomsky's levels of adequacy) or implicitly in the practice of theory development, such as to allow for effective decisions between competing frameworks. The question raised will be whether such criteria reflect answerability of theories to the "properties of the real object of the real world that we are investigating", or whether they are theory-internal criteria proper to the assessment of formal calculi *qua* calculi. The suspicion to be further substantiated, what we have referred to as a skeuomorph effect, is that the putatively real properties and constructs claimed to pertain to natural language are, rather, the results of a Fregean/Galilean attitude to the formal systems that are at the centre of theory construction. The properties of the formal system are transposed onto the object of study, indeed institute that object of study as an artefact which is mistaken for some object of the real world.

In brief, the central burden of our argument will be to make the case for the appropriateness of an instrumental construal of those linguistic theories that take on board the set of base assumptions and methodology that founds the Chomskyan paradigm.

Footnotes.

[1] A. Eddington (1938, pg. 123). Cited in P. Carr (1990, pg. 68).

[2] The position that affords this observation is most frequently referred to as the Duhem-Quine thesis (P. Duhem, 1951 (originally 1906) and W. V. O. Quine, 1953).

[3] A. Eddington (1927, pg. 281). Cited in P. Carr (1990, pg. 68).

[4] Except, perhaps, in the case of some species of modish literary criticism.

[5] W. Kneale and M. Kneale, (1984). A similar observation is made by Quinton (A. Quinton, 1973).

[6] In the two-slit experiment, an electron is fired at a screen with two slits. If detectors are placed at each slit, then the electron is observed to pass through one (and only one) slit - it behaves, under observation, as a particle. However, with just the same apparatus, except for different detectors, the electron behaves as a wave process, passing through both slits. What we have is an electron as both particle and, paradoxically, wave. It is this paradox and the concomitant sense that things are awry that is the basis for Bohr's anti-realist/instrumentalist Copenhagen interpretation of quantum mechanics.

[7] H. Putnam (1981, pg. 49).

[8] G. Lakoff (1987, pg. 262).

[9] H. Putnam (1987, pg. 52).

[10] S. Bromberger (1989, pg. 80).

[11] P. Carr (1990, chapter 1).

[12] P. Carr (1990, pg. 28).

[13] N. Chomsky (1982, pg. 33), here instrumentalism is under the guise of "operationalism." Cited in P. Carr (1990, pg. 47).

[14] As an example of mainstream scientific dissension from this view see Stephen Hawking's remarks on the prospects for a unified theory. (S. Hawking, 1988). Similar doubts are raised by Cohen and Stewart (J. Cohen and I. Stewart, 1994). In philosophy, as well, instrumentalism, in some form or other, is very much a going concern, perhaps even a majority view. (See, for example, B. van Fraassen (1980).)

It seems a fair observation, however, that the actual working practice of most scientists is consistent with an assumption of a realism. This does not, however, always seem to carry over to their musings on their days off.

[15] ***Passim*** but see, for example, N. Chomsky (1980, pg. 187 ff.).

[16] For example, Peirce is of the view that "reality depends on the ultimate decision of the community" (C. Peirce, 1931 - 1958, 5.316, cited in C. Hutton (1990, pg. 11)) and that "the opinion which is fated to be ultimately agreed to by all who investigate is what we mean by the truth, and the object represented in this opinion is the real"(C. Peirce, 1878, cited in S. Blackburn, 1994, pg. 280).

[17] See L. Laudan (1981).

[18] D. Papineau (1996, pg. 306).

[19] *Ibid.* pg. 306.

[20] *Ibid.* pg. 307. Unfortunately, Papineau neglects to specify what "adequate evidence" might be. Rather, he relies on an intuition of the differences between the availability of evidence in some fields, such as paleoanthropology (difficult) and chemistry (relatively easy).

[21] A heliocentric system had been proposed in Hellenistic times, but the view found few adherents, presumably because of its flagrant abuse of common-sense.

[22] It is interesting to note that both Copernicus and Kepler exhibited distinctly Pythagorean, mathematical mystagogic tendencies. What Copernicus objected to in the Ptolemaic model was not that it didn't work (it did and better than the model Copernicus suggested in its place), but that it was not how the universe ought to be assuming a Creator with a proper feeling for mathematical beauty and simplicity (all those epicycles). Similarly Kepler "went in quest not of the ways of nature but of the harmonies" (D. Boorstin, 1983, pg. 308).

[23] J. A. Fodor (1985).

[24] For example, see F. Newmeyer (1983, pg. 42 ff). Russell (1987, pg. 230), amongst others (e.g. D. Steinberg, 1983), charges Chomsky with maintaining, in practice, a position exactly analogous to one in which the range of admissible data is stipulated.

[25] F. de Saussure, (1916, 23).

It should be clear from our previous remarks that we hold Saussure's first claim (regarding any pre-theoretical givens) as disputable, even though there does, at first intuitive blush, appear to be a certain content: the linguistic case is rather less of a given than, say, the domain that physics, at least physics at the macro level, deals with. However, for present purposes our emphasis is on his observation regarding how the objects of some domain might be "considered from different points of view."

[26] Peculiarly, the lack of such an ontological specification need not be as damaging as it might seem. Quine points out that the Greeks had little clue about the nature of the enterprise, nor of its objects, when they devised predictively successful theories of planetary motion. He does remark, however, that while not untenable, "it is a theoretically unsatisfactory situation". (W. V. O. Quine, (1953, pg. 47) cited in J. Katz, (1986, pg. 172)).

[27] A point to be noted about these epicycles was that they were enormously flexible: once one takes the notion on board, then one can endlessly fiddle to make one's model predictively successful, even to the extent of having the primary orbit turn out to be a square (J. Cohen and I. Stewart, 1994, pg. 25). In terms of falsifiability epicycles are the mediaeval counterparts of Freudian suppression and the performance factors of the generative enterprise.

[28] Compare Russell's "distance principle", (J. Russell, 1987, pg. 228). The following may be taken as an attempt to outline the form that such a "distance principle" might take.

[29] The analogy is borrowed from Hutton (C. Hutton, 1993, pg. 168). The same idea is used by Burton-Roberts (1994, pg. 192) to a somewhat different end. He does, however, rehearse the essence of the point in alluding to Magritte's painting of a pipe, beneath which is the message "Ceci n'est pas une pipe." For, of course, what one looks at is not a pipe but a representation of a pipe.

[30] A more or less parallel distinction is Richards' distinction, introduced as meta-apparatus to talk about metaphor, between vehicle and topic (or tenor) (I. Richards, 1936). More recently the distinction has been reinvigorated and elaborated by Black, (M. Black, 1962).

[31] Z. Pylyshyn (1991, pg 239). A complaint made by Pylyshyn, Botha (1979, pg. 45) and Moore and Carling (1982, pg. 68) against Chomsky is of precisely the lack of a sufficiently explicit interpretative function.

[32] R. Matthews, (1991, pg. 196).

[33] *Ibid.*

[34] G. Ryle, (1995, pg. 189). Ryle uses the equator for his example.

[35] Our natural impulse to a referential theory of meaning again.

[36] D. Dennett, (1987).

A similar distinction can be discerned in the idealistic atomism of Leibniz' *Monadology*. For Leibniz the only things that exist are the windowless monads, the stuff of the universe: "outside and between the individual existence of monads there is nonreality." All else, e.g. all relations, "cannot have real existence ... only mental existence." However, "the understanding that thinks relations is the understanding of God. Relations [cf representational constructs] have their substantial reality taken away from them by being referred to an understanding, but because the understanding that carries them is the divine understanding, they ... receive back again a new reality." (G. Martin, 1955, pg. 1 ff., cited in R. Hersh, 1998, pg. 125.) This divine thinking is the grounds for the reality of mathematics: it is the understanding of God, His language in which he figures the world. So, if we believe in Leibniz' God and in this reality for a singular mathematics, then our present worries are by the by: our calculus is merely re-presenting God's thought, which thought constitutes reality. Only, of course, we do not believe in Leibniz' God, and even if we did then we would still have to wonder about which calculus God thinks when he thinks language. Our observation of the Fregean tendency is just the observation of an atavistic reflex to such ways of thinking about formal calculi.

[37] The report is made by Flanagan in T. Honderich (ed.), (1995, pg. 186).

[38] In effect, the requirement is that we accept some formal apparatus as precisely "an object of comparison - as, so to speak, a measuring-rod; not as a preconceived idea to which reality *must* correspond"(Wittgenstein, (1958, section 131)), i.e. not as a transparent window onto the world but as, initially at least, one more object added to the world, and suspend, or are parsimonious with, the

"natural impulse" (Putnam) or "natural attitude" (Husserl) to take on ontological commitments to objects that would excise the apparatus' status as an object of comparison. In Husserlian terms, this amounts to effecting a "transcendental reduction": to defer "the entire ontological commitment that belongs to the essence of the natural attitude, we place in brackets whatever it includes in respect to being" (E. Husserl, 1931, pg. 111). Warrant for a realism amounts to sufficient grounds to erase (some of) the brackets. Picking up on Pylyshyn's remarks (pg. 83, footnote 31), a realist claim requires that one distinguishes between what of the notation is to remain in brackets and what are to be the objects of ontological commitment.

The point, in sum, is that a calculus, no matter how predictively accurate, does not, by and in itself, constitute an explanation.

[39] At this point the reader might wish to resist the implicit comparison with the generativist paradigm by pointing out that on innumerable occasions Chomsky, for one, has emphasized that such perspectives are appropriate; the linguist is not constrained to data from speaker judgements. The common observation, however, (to be illustrated in chapter IV) is that *in practice* precisely this restriction holds.

[40] If left unnegotiable and if further criteria are deemed unnecessary, then an upshot of the second criterion, in effect that a realism is warranted on the basis of inter-theoretic commensurability, would seem to have the effect of making reductionism a condition of a scientific realism.

CHAPTER III.

ONTOLOGIES FOR THE LINGUISTIC I: Non-Psychological Realism.

1.0 Introduction.

Construing a theory's constructs as real requires, at some time or other, taking some stance as regards the ontological type of postulated entities: if we have confidence in the reality of the things that we are talking about, then, presumably, we should be able to state what sort of things they are.[1] In raising this point we are taking up the issues associated with the second and third identified strands of Chomsky's revolution: the move towards a realism and the attempt at an explanation of the linguistic in terms of the mind/brain. It is at this juncture that the Chomskyan paradigm (where it is defined as generativism) bifurcates, where Chomsky's psychologism (a psychologism that is reductionist[2]) parts company from those of an ontologically agnostic, or those of an autonomist or Platonist persuasion.

The central contention that we are to pursue in the following is that this realism can only be achieved, at doubtful best, by taking an autonomist ontological view of the linguistic: Chomsky's psychologism collapses into an autonomism and so the third strand, the reductionist psychologism, with its correlative type of explanation, is unachieved. This "at best" is doubtful because, amongst other things, such an autonomism precludes one primary evidential warrant for a realism: that of the ability of the constructs of a theory to be confirmed in relation to other theoretical domains, i.e. for the constructs of a theory to be observable from some this-theory-external perspective and so excise any skeuomorph effect. What results is an inability to deal with the embarrassment of extensional equivalent grammars, and they are an embarrassment for the autonomist, as they are for Chomsky, because they undermine confidence in the correspondence of the set of constructs and relations of any particular grammar to any calculus-external realities: the upshot is an apparent undermining of any realist claim.[3] One may, suspending this doubt, if one wishes, term the theory interpreted in this autonomous light as "psychological" but only in the sense that the psychological is construed as constituting a level of description that is neutral to, and distinct from, any mind/brain internal states of affairs, i.e. as itself an autonomous, non-reducible domain. Such a construal aligns the Chomskyan

Generativist enterprise with the functionalism that is the standard rationale of the cognitive sciences - competence theories correlating with the level one functions of Marr's hierarchy of levels of description.[4r] Re-applying our doubts, a line of enquiry which we will pursue is that of the preferability, at least in the linguistic case, of an instrumentalist construal of level one competence theories. This will be to exclude such theories from constituting any part of an account of the aetiology of linguistic behaviour: fictions do not have causal powers.

In drawing this somewhat negative conclusion we aim to identify its source in the originary positing of the proper object of study as linguistic competence. We will then (chapter VI) outline a positive proposal which will in effect reconfigure the object of study as a constructive procedure rather than as the outputs, the set of sentences, of that procedure. In effect our proposal will amount to a change in idiom: a language is not so much what one knows as what one does, a case of exchanging the ghost in the machine for the machine in the ghost.

2.0 The ontological options.

The ontological issue, in effect the question of the (non-)autonomy of the linguistic domain, is not a merely peripheral and somewhat esoteric concern best left to philosophers and then forgotten about, but is one which relates to the nature of the explanation that is attempted by linguistics - whether or not linguistics is to be understood, as Chomsky claims, as one of the natural sciences. The views that are taken on the issue are also, as we will outline, correlative with attitudes to the role of the formal apparatus.

Katz points out that, ignoring agnosticism (and don't [see the need to] care - arguably the majority view), the range of ontological options is limited to nominalism, conceptualism (for our purposes this is interchangeable with "mentalism" or "psychologism") and Platonism.[5r]

To pick up firstly on what Katz ignores: an explicitly "agnostic" position - but at least they care - is taken by Pollard and Sag.[6r] They argue that, despite certain philosophical/ontological unclarities, language can be studied profitably without the need for such unclarities to be resolved in advance. Implicit, perhaps, in their argument, is the not unreasonable contention that one of the promises held out by an adequate linguistic theory is that it might well cast a certain retrospective light on such foundational issues.

In support of their leaving of the ontological question in parentheses, Pollard and Sag cite the philosophically problematic status of the theoretical constructs of quantum mechanics. They take this as an example of a domain where both the need for the postulation of such constructs is well-motivated and the predictive success provided by their postulation constitutes good grounds for continuing in the theory, despite the philosophical puzzles these constructs give rise to (for example, wave-particle duality seems to bring into question the law of excluded middle).

It is worth pointing out that this argument, essentially an argument from heuristic fertility, is, both in the case of quantum mechanics and linguistics, entirely consonant with an instrumentalist/anti-realist construal. It is not, by itself, an argument as to why we ought to take a theory realistically, but an argument as to why we ought to continue in a theory on the basis of the productivity of the epistemic access that a theory affords: some talk, particularly if it is in some manner truth-bearing, is better than none at all. It is interesting to note, however, that what gives rise to the various non-realist interpretations of quantum mechanics is precisely the philosophical anomalies that accrue, the "unquestionable success" of the theory notwithstanding. Amongst the interpretative options that have been proposed are Einstein's, that quantum mechanics "is at best an incomplete description of a better-behaved classical underlying reality", i.e. we haven't got it right yet, and Bohr and Heisenberg's explicitly anti-realist Copenhagen interpretation in which:

> there is no deep quantum reality, no world of electrons and photons. There is only description of the world in these terms: quantum mechanics affords us a formalism that we can use to predict events ..., but it is misguided or senseless to postulate a quantum reality answering to the description. Problems such as the wave-particle duality ... suggest that there is no reality behind our observation.[7r]

A similar rationale for a questioning of the realist credentials of (the objects of) linguistic theory underpins our present project: if similar conceptual problems and anomalies result from Chomsky's generative enterprise, then this would motivate, at the very least, the consideration of some non-realist perspective and, assuming the viability of an ultimately realist construal of some linguistic theory, a potential correlative revision of foundational assumptions.

Agnosticism aside, the first entry in Katz' catalogue (nominalism) is dismissed as a non-starter, because it restricts the linguist to the study of linguistic tokens, i.e. actual utterances, from which study it is not possible to do justice to the linguistic system which is always in excess of any set of actualized sentences. Katz' further entries are guilty of somewhat simplifying the situation, for there are positions which, while eschewing the conceptualist/psychologist option, also attempt to avoid a full-blown Platonism, a case in point being Carr's Popperian autonomism.[8] Arguably, the issue boils down to what one does with abstract objects; how one negotiates the abstraction of sentence types, for it is the consensus that these types constitute the immediate objects of the linguist's consideration,[9] these being distinguished from utterances/tokens: it is only this latter that are unproblematically spatio-temporal and concrete. Conversely, a lot rides on what you get to call "psychological" and the extent to which the psychological gets to negotiate the abstraction of the linguistic.

Across the range of ontological opinions the view is that the linguistic, as to be defined by some suitable theory or set of theories, constitutes a nomological domain, the laws of which are *sui generis*. The disagreement, as sketched above, comes in over the type of things that linguistic entities are: the Chomskyan position is that they are psychological entities, having their properties by virtue of the informational structure of the innately specified principles and parameters of the I-language, which I-language is realized by "presumably neural mechanisms" thus holding out the promise, ultimately, of contentfully construing linguistics as a branch of biology;[10] the Platonist/autonomist holds that the linguistic cannot be reduced, nor explained in relation to the psychological, nor to any other nomological domain; in short, the linguistic stands outside of a reductionist's hierarchy of nature (see next section): in respect of that hierarchy, the linguistic is anomalous thus making of linguistics a special science and not, *pace* Chomsky, one of the natural sciences.[11]

As is routinely acknowledged, science is engaged in attempting explanation. As we noted earlier, Aristotle suggested that the mark of proper understanding, the mark of an explanation, is to know not only what is the case but also *why* it is the case. The putative status of linguistics as a special science restricts linguistics to meeting only the former of these conditions, to what we might term a "constitutive explanation"[12r]: we are excluded from not only achieving, but even from attempting, an account of why the (natural) linguistic is as it is. To illustrate, and to relate our present concerns with those of the previous chapters (regarding the need and means to escape closed-circuit functioning), we need to make another short astronomical detour.

3.0 Constitutive explanations, reductionist explanations and the continuity of the levels of nature.

To the ancients the planets were a discrete natural kind, a discrete object of study to be accounted for by a theory which differentiated their behaviour, their motion, from that of the background stars, indeed the etymology of the word suggests the observed distinction: planets are "wandering stars" which do not follow the same regular pattern of movement as do stars in general. The Keplerian account provides a very nearly entirely accurate, predictive theory of their motion in terms of mathematically formulated laws. The laws express the generalizations which allow for the confirming predictions. One can say, without doing violence to the language, that the Keplerian laws explain the motion of the planets, because the planets' motion across the heavens is predicted by and conforms to those laws: what constitutes planetary motion is the conforming to the laws as expressed by some mathematical formalism and any, as it were, "extensional equivalents" of that formalism.

What we have here is an answer to the question of what constitutes a particular domain; what are its entities and its laws. In having been provided with such an account, we may be led to ask for a further explanation of why the behaviour, in this case planetary behaviour, is as it is. In this instance we have an answer (Newton's) and the nature of that answer illustrates a form of explanation which we can term reductionist. The central point is that the laws that govern planetary motion are themselves explained in terms of a more basic and more inclusive set of laws: the planetary orbits and their approximately elliptical nature are the result of a set of interacting gravitational forces. What we have is a reduction of one set of laws (Kepler's) and the correlative domain to another, more inclusive set (Newton's) and domain. In general terms, and what is exemplified in our illustration is that "reductions are explanations of phenomena described by one theory in terms of the phenomena described by a more basic theory."[13r] It is important to recognize what this does not mean. It does not mean that the less basic phenomena are explained away, nor that the theory at the less basic level is disproved or somehow discredited. Rather, what we get is an explanation for the less basic level in terms of the laws of a more inclusive level, and what we also get is, in effect, a mutually confirming dialectic between theoretical domains.

To add some flesh: the Keplerian laws do no more (nor no less) than characterize the motion of the planets. In (almost) accurately providing that characterization they leave unanswered the question of why the planets conform to

those laws. (Kepler might demur here, but his answer, I suspect, would have been that of a mathematical mystagogue citing "God as geometer.") To achieve an answer it is necessary to invoke an ontological hypothesis as to what sort of things the planets are (i.e. massy bodies, as opposed to, for instance, deities or harmonics of the spheres made visible - these may be non-starters nowadays but they did have a certain currency), and a theory that is about massy bodies: a theory of gravitation. Having got this right (or more or less) on both counts we then achieve an explanation which is (sort of) causal: what (sort of) causes the elliptical motion of the planets around the sun - the reason why - is the interaction of the gravitational forces exerted by all of these massy bodies.[14] Precisely the point of Chomsky's psychologism, articulated by way of a mental representation hypothesis, is that it is in attempt to provide an account which is (literally) causal (or part of a literally causal account) by virtue of holding out the promise of relating the abstraction of linguistic "laws" to the neurophysiological substrate, which substrate has (literally) causal powers and which gets to be quantified over as a physical world entity (note that Keplerian laws are not the sort of thing that, because they are abstract, either have causal powers or get to be quantified over as entities of the physical world). In brief, the Chomskyan strategy for achieving an explanation, and the manner in which linguistics gets to be one of the natural sciences, is, at some level of discrimination, to quantify over the *abstracta* of a calculus, the terms and relations that express the laws of the domain, such that they are taken to correspond to *illata* (the mental representations) which are involved in the aetiology of linguistic behaviour: "The statements of a grammar ... are statements about structures of the brain... These structures are specific things in the world".[15]

Picking up on the issues of the previous chapter, it is also worth noting how, in our illustrative case, (chosen as an exemplar of a theory with a best claim to a realist construal,) the separate hypotheses involved get to be mutually confirming: that the planets' motion is predictable from a theory of gravitation confirms them as massy bodies, and their being massy bodies conforming to certain laws that are themselves predictable from a theory of gravitation gets to be confirmatory of that theory of gravitation. Of course, that need not be the end of the chain of "why" questions; one can quite reasonably re-apply the question and ask why gravitation is as it is and, having come up with an answer, ask again about that answer. Explanation must, however, have a stop: in the end, to the question "why?", the answer is the deeply mysterious, "because that's how it is", either that or, which amounts to the same, invoke the Deity. At root, what is disconcerting about non-reductionist, autonomous

positions is that, in abruptly cutting this chain of questions, they appear impatient of God's, or some equivalent's, appearance.

The progress towards wider and more inclusive accounts may be figured as a hierarchy of lawful domains, the higher ones subsuming the lower and having as consequences those less inclusive laws. What emerges is a distinguishable "continuity of the levels of nature", the basis of the reductionist's credo, in which:

> reality ... is a multiple hierarchy of levels of nature, each level marked by a nexus of nomic generalizations and supervenient on all those levels below it on the continuum.[16r]

For further example: the law that relates the parameters of the macro-phenomena of the pressure ("p"), volume ("V") and temperature ("T") of gases is expressible, that is characterizable, through the equation: p x V = c x T (where "c" is a constant). This law is itself to be explained through, i.e. can be derived from, the laws that capture the micro-phenomena of the behaviour of the individual molecules. These latter laws subsume and explain the former and relate the behaviour of gases to a wider range of phenomena: molecular behaviour in general. To revert to our original example, a theory of gravitation ties planetary motion to all other phenomena that are provided with an explanation by that theory: planets, surprisingly, do what they do for the same reasons that apples do what they do when they detach themselves from the tree and they do this, even more surprisingly, for the same reasons that the tides rise and fall.

This tying together of apparently disparate phenomena, (i.e. phenomena that pre-theoretically might appear to be in discrete domains, needing distinct explanations,) under the cover of one theory is an instance of "consilience".[17] The notion is related to the parsimony of Occam's Razor: when a theory ties phenomena together it achieves a singular explanation, and so obviates the need for several disparate but less encompassing accounts. The consilience a theory achieves is a mark of some underlying unity. However, a condition on a law or theory applying to a phenomenal range is the applicability of that law or theory to the types of entity, the natural-kinds, that constitute that phenomenal range. For instance, if an entity obeys the law of gravity, it must be of the same type, in some crucial respect, as all other entities to which the law applies. Conversely, failure to obey gravity would mark an

entity as being of a type, or natural kind, that is distinct from those that do obey the laws of gravity.

The point of making these remarks is that it allows us to define what is to be meant by "reductionist psychologism". The "psychological" is definable as a nomological domain, a sort of logical/notional space, that is inhabited by that set of events and entities that conform to a certain set of laws. The linguistic is a subset of the psychological (or the biological) if and only if the set of events and entities that constitute the linguistic domain are also subject to some set of laws that apply to events and entities, other than the linguistic, which constitute the logical space of the psychological (or the biological). If there are no such covering laws, then the linguistic is an autonomous domain vis-à-vis the psychological. It may be noted that this formulation does not specify in advance what is and is not psychological, rather the psychological is revisable relative to the formulation of laws in theorizing in much the same way as the notion of matter has been revised and broadened relative to the postulation of successful theories.[18r]

More generally, one mark of the non-autonomy of a domain and its integration into a hierarchy will be, precisely, such achieved consilience across domains. Such consilience, in being strongly confirmatory, is, for the same reason, warrant for a realism in regard of the constructs of a theory under test. Moreover, and crucially, a condition on consilience is that the constructs of one theory are, as it were, observable from the perspective of the theoretical domain with which it is commensurable. The misgiving we raised about linguistic theories was that the data were interpreted theory-internally. When a theory and its constructs are shown to be commensurable with those of other domains, this supplies a certain theory-externality; we have, as it were, a different pair of spectacles and a warrant for realism. While the perspective is not theory-neutral, it is at least neutral of the theory one is attempting to adduce evidence for. It is precisely such theory-neutrality that would annul suspicions of a theory's posited entities being skeuomorphs induced by the conceptual apparatus that is applied to in the process of hypothesis formation.

These observations pertain to the question of what is to be taken as the relevant data for confirmation of linguistic theories. The moral to be drawn is that:

> the data relevant to the confirmation of [a theory] T include the data predicted by the conjunction of T with any other theory that is independently well confirmed. In particular, they are not exhausted by the entailments of T taken alone.[19r]

In other words, if a set of constructs are real, then their observability will not depend on the epistemic access provided by the theory that posits them and which interprets the data. This is a point that is rehearsed severally in the literature:

> If the [realist and mentalist] proposal is right, then linguistic behaviour and linguistic judgements do not exhaust the evidence on which linguistics should or can proceed. Other phenomena currently studied ... will also be pertinent. The study of natural languages becomes, in this respect at least, very different from the study of formal languages, and linguistics cannot simply confine itself to special versions of issues appropriate to such languages.[20]

More particularly, the claim that linguistics is a natural science, which claim is made on the basis of a putative commensurability with psychology, requires the relevance of, precisely, psychological theories and the brain sciences in general. In this regard, and a symptom of his reductionist psychologism, Chomsky remarks that:

> In principle, discoveries about the brain should influence the theory of mind, and at the same time the abstract study of states of the language faculty should formulate properties to be explained by the theory of the brain and is likely to be indispensable in the search for mechanisms. To the extent that such connections can be established, the study of the mind - in particular, of I-language - will be assimilated to the mainstream of the natural sciences.[21r]

The assumption of the continuity of the levels of nature and a correlative ontological parsimony underlie, indeed constitute prejudices of, philosophical materialism. They are, however, prejudices which have a certain inductive warrant: precisely these prejudices are confirmed by the success of the scientific enterprise of the last half-millenium and the resulting cogency of the possibility, in physics at least, of some grand unified theory of just about everything. More particularly, they are, in respect of the linguistic, prejudices that take heart from the direction that the enquiry into the mind/brain is taking. It is now, apparently, consensual that mental phenomena as seemingly quixotic as moods can be correlated to the presence or absence of neuro-chemicals. In a similar vein, a major growth area appears to involve the correlation of behaviours with the presence or absence of specific genes. For example, in the case of

William's Syndrome, specific cognitive deficits are correlated with a genetic, chromosomal disorder.[22] More generally, it appears that we are beginning to achieve some understanding of the more detailed functioning of the brain, where that understanding recapitulates just such a reductionist's hierarchy of levels:

> The auditory system is a fair example. There is evidence that the auditory cortex displays two-dimensional columnar organization: columns of variously specialized cells arranged along one axis respond selectively to frequencies indicated by incoming impulses from the auditory nerve, while columns roughly orthogonal to these somehow coordinate input from one ear with input from the other. The particular sensitivities of the specialized cells is to be explained in turn by reference to ion transfer across cell membranes, and so on down. For its own part, the auditory cortex interacts with other higher-level agencies - the thalmus, the superior colliculus, and other cortical areas - which interactions are highly structured.[23r]

Even more suggestive of the viability of explanations of linguistic functioning at the level of neurology and genetics are, as regards the former, studies of the correlation between various locations of brain lesions and particular linguistic deficits, and, in the latter regard, studies of family (i.e. genetically related) groups whose members evidence certain systematic language (e.g. morphological) impairment.[24]

In the light of this random sample of how an understanding of the mind/brain is being developed, and of how it is offering understanding of phenomena dependent on mind/brain functioning, an enquiry that results in the conclusion of the incommensurability of the objects of a linguistic theory with more basic or adjacent properties of the mind/brain is perplexing, even to the extent of being a *reductio ad absurdum* of realist claims made for such a linguistic theory. This because, in an autonomist's/Platonist's view, we have to say that whatever it is that the neuro-linguist is talking about has nothing to do with what the linguist proper is talking about: east is east and west is west and never the twain shall meet. We get a strange, if not downright bizarre, bifurcation between two domains and sets of objects where each domain/set seems to make sense only as related to the other (do we really want to say, along with Katz, that there are languages irrespective of there ever being speakers of

those languages?), but then we are told that, by virtue of their different ontological status, the relation they are in is, on the one hand, not part of the explanation of the linguistic, and, on the other, this relation is, as we shall see, only explicated by recourse to an account that takes refuge in mysteries.

While such considerations supply a motive for continuing in Chomsky's general, psychological and reductionist project, (for one thing, a psychological story promises to relate and assimilate the two sides of this dichotomy,) these considerations do not by themselves provide psychological content to some linguistic theory nor confer, to use a much abused phrase, psychological reality on that theory's constructs. The conclusion, which it is the purpose of these chapters to draw, is that the Chomskyan paradigm has not only not itself achieved such psychological content, but also denies, by virtue of some of its initial foundational assumptions, the very possibility: given the initial set of assumptions and positing of an object of study, the autonomist/Platonist conclusion is unavoidable.[25] However, this conclusion with its correlative and disconcerting (east is east, west is west) ontological bifurcation rests on a realist construal of linguistic theories and their constructs. As we pointed out, realism is not the only option. One *prima facie* motive for examining the possibility of an instrumentalist perspective is that it would absolve us from just this disconcerting bifurcation and correlative ontological commitments. However, as we have just noted in respect of Chomsky's psychologism, a motive, no matter how persuasive, is not itself sufficient to realize what is desirable. In the following we aim to provide precisely such grounds as argue for an instrumentalist interpretation of generativist theories.

4.0 Autonomism, Platonism and the (dis-)continuity of the levels of nature.

4.1 Arguments for a non-psychological ontology.

Central to the autonomist/Platonist position is the non-reducibility of linguistic theories to any other nomological domain. Consequently, other domains and their corresponding data are, and must be, irrelevant to disconfirmation of linguistic theories. There are, at least, three ways one might come towards stating this claim: one could simply, as Fodor characterizes the Platonist position,[26r] stipulate one's interests as being confined to the mathematical problem of formally specifying a grammar. If one restricts one's interest in this way, thereby restricting oneself to an

exclusively constitutive account, one may also stipulate one's data (in this case, speakers' intuitions; a parallel would be naked-eye observations of the planets' apparent motion). What one is then concerned with is the calculus *qua* calculus as an instrument for achieving coverage, i.e. to predict acceptability judgements. What one is not doing is attempting an account of why the phenomena are as they are, for example by relating the linguistic to properties of the minds/brains of speakers.

However, Fodor's characterization of the Platonist/autonomist option as mere, unmotivated stipulation of the irrelevance of the psychological is not an entirely fair representation of that option. The taking of the Platonist stance is based on arguments of two basic types. On the one hand, arguments are adduced to the effect that, given the posited object of study (construed as the set of sentence types of a language), then this object's properties make it incompatible with a mentalist ontology: types are abstract and so not commensurable with a physical mind/brain; the second line of argument is to point to the incompatibility of the infinitude of linguistic objects with the finitude of the mind/brain.[27] An alternative tack is taken by Carr.

Recall that Chomsky asserts that a condition on linguistics being "assimilated into the mainstream of the natural sciences", a condition on content being provided for the psychological claim, is that "connections can be established" between the psychological/neurological and the linguistic. Conversely, the inability to achieve such connections might be suggested as grounds to suspect the propriety of a reductionist psychological ontology for the constructs of linguistic theories. On the other hand, just this inability is predicted - a prediction Carr takes to be confirmed - by an autonomism/Platonism: the data are restricted, because of the putatively non-reducible *sui generity* of linguistic objects, to speaker-hearer judgements.[28] Carr points out that no matter which ontological position one takes the data are, *in practice*, just so restricted; indeed:

> In Chomsky's case, we find the rather odd situation in which he allows that other sorts of evidence are relevant to testing but never in practice uses, or recognizes, such evidence.[29r]

There is, in respect of the types of data applied to, no methodological distinction between linguists working from either ontological perspective, nor between these and those who take an agnostic position.[30] There does, however, appear to be an "evidential distinction" between linguistic and psychological enquiry. Carr takes this

"methodological divide" between the linguistic and the psychological to require an explanation.

The explanation (Carr's) is that the two discourses reflect a real, ontological distinction in the nature of things: "the divide exists because the world is actually differentiated in this way."[31r] There is the psychological (in the guise of, for example, the psycholinguistic) and there is the linguistic, they are "distinct sorts of enterprise", and are so because their objects of enquiry are of different and incompatible ontological types. The former is concerned, because of its drawing of its primary data from experimentation, with spatio-temporal events; the latter, because it is concerned not with utterances but with sentence types, is not. (Types are to be regarded as abstract objects which, by definition, are neither spatial nor temporal.) Consequently, east is indeed east and west is indeed west:

> Autonomous linguistics is thus methodologically distinct from natural sciences in that its general statements are not propositions about events, and are thus not laws in any [causal] sense ....[32r]

The upshot is that linguistics, because of the nature of linguistic objects, is excluded from passing beyond a constitutive account. The perceived evidential distinction, itself based on a fundamental ontological distinction between the kinds of things that are the objects of investigation in linguistics on the one hand, and in the natural sciences on the other, is taken to be confirmatory of autonomism and disconfirmatory of Chomsky's reductive psychologism. What we also get is the ontological anomalousness of the linguistic, at least it is anomalous from the perspective of the natural sciences; it becomes, rather, a special science. Carr contrives to portray this ontological anomalousness as a positive result, one that "avoids the pitfalls of reductionism", amongst which pitfalls is "an impoverished conception of ontological diversity."[33r]

This "pitfall", however, is no more than a rhetorical dressing up of mutton as lamb, or rather, lamb dressed down as mutton: one could say, for the contrary position, and with much better foundation, that it avoids the pitfalls of anti-reductionism, amongst which pitfalls is a gratuitously extravagant conception of ontological diversity. We take the Quinean line here where less is distinctly more and the virtue is, all things being equal, in getting away with as parsimonious an ontology as possible.[34] This aside, in taking a Popperian view on the ontological status of the linguistic (for more on this see below) we are able, Carr claims, to avoid "the excesses

of Platonism" and, indeed, see the anomaly as not anomalous at all because the anomalousness only comes in by way of a mistaken prejudice for, and unwarranted assumption of, reductionism.

4.2 Problems with Platonism.

The unpalatabilities of a Platonism, whether it be in mathematics or linguistics, are, several. Firstly, no story at all is, nor can be, offered as to how platonic entities come to be - they are timeless, necessary, causeless and uncaused, one is reduced to stating that they simply subsist; there can be, seemingly by some act of stipulation, no answer to any question as to why they are as they are. Second on this agenda are the ontological excesses that Carr alludes to above. One consequence of Platonism in linguistics, and in general, is an immediate and unchecked proliferation of abstract entities. One of the disconcerting corollaries, in the linguistic case, is that not only do the sentence types of existing natural languages subsist, but also all the sentence types of languages (and so the languages) that are no longer in use, the sentence types of languages that are not yet in use, and the sentence types of languages (indefinitely many of them) that neither are, have been, nor will be in use. Such a situation is profligate in the extreme, to the extent of being downright squalid: it is, in Quine's words in respect of a similar position, "rank", "a slum" and "a breeding ground for disorderly elements."[35] What is offensive is the lavish violation of Occam's Razor (which Quine picturesquely figures as shaving Plato's beard), not to mention the assault on common-sense. Furthermore, it would appear that Katz' position seems to have the unfortunate corollary of making the linguist's objects of study at best problematically distinguishable from objects with the "property" of non-existence.

As remarked, Katz is committed to quantifying over natural languages both actualized and unactualized. The fact of their actualization, through the agency of human minds/brains is merely contingent; it is irrelevant to their existence. But if the fact that the actualization of a language is merely contingent, then how does one get to demarcate a linguistically proper and linguistically non-contingent distinction between natural languages and the set of languages - imaginable and unimaginable - of which the natural are some subset? Take the contingencies of the human mind/brain away and the linguistic domain becomes considerably and (here's the problem) indiscriminably richer. Presumably, this task of demarcation is one for some enquiry, only it is not a properly linguistic enquiry for it would need to apply to linguistically

contingent facts, i.e. regarding, presumably, the human mind/brain. But, this seems to draw a map of the linguistic domain which, other than some small, contingently actualized subset, is empty (but in reality non-empty), and its non-emptiness indicated by the legend Here there be dragons, i.e. languages both imaginable and unimaginable. The trouble with dragons is that they are indistinguishable from entities that are non-existent. If actualization is contingent, then we need to cast around for some other touchstone of existence. Take actualization and minds/brains away as touchstones of existence, then what is the difference between our objects of study as things that exist and objects of study that do not exist? I think that we need to be told.

The third, and similarly unpalatable, corollary of a Platonism is the perennial problem of how non-spatio-temporal, abstract objects get to be in some relation (i.e. a knowledge relation) with spatio-temporal minds/brains: platonic objects are:

> beautiful (at least to some), imperishable, multitudinous, intricately connected. They toil not, neither do they spin. Nor, and this is the rub, do they interact with us in any way. So how are we supposed to have epistemological access to them? To answer, 'by intuition', is hardly satisfactory. We need some account of how we have knowledge of these beasties.[36r]

Katz gets himself into knots on this topic.[37] In effect he recapitulates a Chomskyan innateness hypothesis that looks far less plausible than the original: our intuitions about abstract objects are taken to be internally constructed representations which representations may (or may not[38]) correspond to the mind-external abstract objects which they intend. This representational faculty is innate. But given that these objects have no causal powers (by virtue of not being spatio-temporal), then how come we have this faculty of internally constructing representations of them? There can be, seemingly, no evolutionary story (except, perhaps, extreme and serendipitous mutational accident), nor any causal story: it must be simply a mystery.

Similar difficulties arise in respect of the need for an account of the facts of language change. Presumably, the process is, in some way, caused, but Platonic entities are not the sort of things that get caused, nor are they causal. On the other hand, minds/brains are, again presumably, just the sort of things that enter into causal relationships. Consequently, it would seem likely that to account for many of the phenomena associated with natural language it will be necessary to invoke properties of the mind/brain. Even if it should turn out that there is some proper content to the

notion of the linguistic as ontologically distinct of the mind/brain, and this shown to constitute a proper province of the grammarian, it will still leave not only room, but the need for the "psychogrammarian" to attempt to understand how we come to be language using beings. And this is, in Fodor's words, where the interest and "the action is[:] all at the other [i.e. conceptualist's] end of town."[39r]

Viewed from this angle, what Chomsky's programme amounts to is the placing of a bet that, once one has exhausted the study of language from the psychological perspective, then there will be no further linguistic phenomena to be accounted for: the psychogrammarian will subsume the grammarian and, correlatively, (and this is why it is so appealing,) there is no ontological bifurcation with its attendant problems. Given these problems, why not stick with an innateness hypothesis, some faculty which constructs representations, and take that faculty and those representations[40] as the proper objects of study, indeed the only objects of study, and do away with these highly problematic abstract entities that are, if they exist at all, both themselves inexplicable and obstructive of an explanation of the human language faculty? This is, very crudely, the line that is taken, and for broadly similar reasons, by constructivism/intuitionism in mathematics.[41] However, and also crudely, one reason why constructivism/intuitionism is, I take it, a minority interest in mathematics is that it seems the abstraction and correlative autonomy of mathematical objects will just not go away, in much the same way as, conversely, and on the grounds cited above, the reduction of the linguistic, defined more or less on Chomskyan lines, to the psychological, as we will see, remains unachieved.

4.3 Autonomism and Popper's "world three".

The Popperian perspective, Carr insists, allows us a realist discourse about linguistic objects, taken as autonomous and abstract, without involving us in the same problems as make a linguistic Platonism so unpalatable. For one thing, by positing a non-contingent relation between language and mind, mediated by a notion of emergence, we get to negotiate the ontological extravagances of Platonism that we adverted to above. However, it is not clear to what extent Carr's Popperian position can be distinguished from the brute claim that there is a domain of mind-external, autonomous, objective knowledge, together with the assertion that this domain interacts (we are not told how) with psychological states of individuals, as if you can

get the requisite interaction by fiat, or by some spurious necessity, because it is the only way to make sense of the position.

Oddly, however, and a second[42] reason to suspect that Chomsky's psychologism might not be contentfully distinguishable from an autonomism is that, in certain respects, the picture that emerges is not dissimilar to one which is, interpretably, Chomsky's: in his case mind-*internal* tacit knowledge, i.e. the I-language, (captured at some suitable level of abstraction above the actual instantiating mechanism) interacts with conscious psychological states of individuals and it is this knowledge which is putatively involved in the aetiology of the behaviour. For example, it is supposedly what is accessed when we make some judgement of acceptability. But just how this interaction happens we are not told; it is left as a problem for the brain sciences. (We may note that, at bottom, the issue is a rehearsal of philosophy's mind-body problem, that of just how one is to accommodate (or relate) the abstraction of the mental within (or to) properties of physical entities. In broad terms, this conceptual and ontological bifurcation/dualism is what the mental representation hypothesis is supposed to negotiate.)

This problem is not confined to linguistics, but is replicated in cognitivism in general. Fodor observes that a characteristic of cognitivism is its reliance on "neo-Cartesian" explanations that appeal to some body of mentally realized declarative knowledge in account of the nature of behaviour. Such explanations Fodor takes to be "enthymemic", i.e. one premise is left implicit. The "premise" being the nature of the mechanisms that take an individual from the body of declarative knowledge to the realized behaviour; i.e., in linguistic parlance, the performance (in Popper's terms, interactive) mechanisms which:

> do for Chomsky some of what the pineal gland was supposed to do for Descartes: they are invoked to answer the question "How does the structure of behaviour come to mirror the propositional structures that one cognizes?"[43r]

The primary difference between Carr's Popperian position and Chomsky's is the ontological status of the body of knowledge: in the Popperian case it is mind-external, in the Chomskyan case it is mind-internal. As in the Popperian case, it is the shadowy and unexplicated nature of this interaction that will be a cause for concern and which makes the object of the Chomskyan study resistant (*pace* Chomsky's protestations) to integration into the natural sciences.

Central to the Popperian scheme is a division of the world into three ontological domains: worlds one (the physical), two ("subjective experiences", the psychological) and three (objective knowledge, the objects of those experiences).[44] It is amongst this latter that Carr encourages us to place linguistic objects, which objects are, *contra* Chomsky, not elements of individual psychology, but are public and speaker-external. Although we are not here concerned with the propriety of this ontological catalogue, we are concerned with how successful it is in addressing the unpalatabilities of a Platonism: the irreducible otherness of the ontological category of abstract objects, and, secondly, the relation between those objects and individuals.

On the former count, the solution is to see each of the latter two worlds as constituting ontological categories that are emergent from the ontological category that precedes them in the series. Rather than be committed to a mysterious pre-existing collection of subsistent platonic entities, these entities and their properties are emergent[45] out of some creative evolutionary process:

> I suggest that the universe, or its evolution, is creative, and that the evolution of sentient animals with conscious experience has brought about something new. .... With the emergence of man, the creativity of the universe has, I think, become obvious. For man has created a new objective world of the products of the human mind.[46r]

Furthermore, these emergent (types of) entities and their properties are irreducible to, that is cannot be predicted from, the properties of the evolutionary prior domains:

> In a universe in which there once existed (according to our present theories) no elements other than, say, hydrogen and helium, no theorist who knew the laws then operative and exemplified in the universe could have predicted all the properties of the heavier elements not yet emerged, or that they would emerge; or all the properties of even the simplest compound molecules such as water.[47r]

The problem with this latter claim is that it is not at all obvious that it is true, and even less obvious how it could be shown to be true (or false). It appears to invoke as much mystery as Platonism, but in this case the mystery is over how these new properties emerge in a kind of "evolutionary saltation".[48r] The relation between higher and lower

level appears to be one of some sort of inexplicit, because unexplained, dependence or "supervenience". To say that some type of entity or some set of properties is supervenient on some other type of entity or set of properties is to say that there could not be any differences in the former without there also being differences at the latter substratal level. However, the same entities or set of properties might result out of a substrate with different properties. The trouble is that what we get out of the notion of supervenience is a label that does justice to an observation of what is to be explained, but the label can not do service as an explanation. As Blackburn remarks:

> One promise the notion [of supervenience] holds out is that by its means we can understand the relation of ... different layers of description without attempting a reduction of the one area to another. [But] the value of this promise depends on how well we understand the supervenience relation itself. If it is a dangling, inexplicable, metaphysical fact ..., then supervenience inherits rather than solves the problems of understanding the various areas.[49r]

Furthermore, there are many cases, such as in our illustration of the gas laws (pg. 98), where more basic level laws can be shown to be predictive of the laws that pertain at some higher level. The quite general observation, cutting across domains, particularly those domains in which progress has been made, is that "autonomy assumptions have not proven viable".[50r] Indeed, it is arguable that a condition on the revisability, and so development of theories, is the ability for one theoretic domain to be revised in the light of theories in adjacent domains. The history of science furnishes many examples of such co-evolution with which goes a correlative revision and reconfiguration of the categorizations and conceptual schemata that give access onto phenomena.[51] A corollary of an assumption of a domain's autonomy is precisely a resistance to such a reconfiguration. The upshot is a species of "conceptual necessity" (recall, this is Chomsky's phrase, predicated of Minimalism's competence/ computational system) whereby the terms and categorization with which we approach a phenomenon take on a degree of unrevisability, and an unrevisability which is reinforced by the concomitant insulation of theories from data from other theoretical domains.

What Popper requires, if we are to believe him, is some way to show, rather than baldly assert, that, for some principled reason, the apparent autonomy of a domain is ineliminable by any future scientific development. This appears to be the

burden on all anti-reductionist positions. Given the above observed, quite general non-viability of autonomy assumptions, it would seem that autonomy of a domain is the exception rather than the rule: it is a surprising, one might even say, anomalous result. Furthermore, whereas the evidence for the non-autonomy of a domain is just the mutually confirming dialectic between theoretical perspectives we outlined, the evidence for autonomy is just the lack of precisely such confirmatory evidence of a theory's constructs from any external theoretical perspective.

The upshot is that any theoretical position (e.g. autonomous linguistics) that claims the irreducibility of its domain to any other, or any non-autonomous position that fails to realize evidence for that non-autonomy (e.g., we shall argue, Chomskyan linguistic theory), looks like simply the failure to get it right. This either, guiltlessly, because there is no suitably developed (or, on the autonomist story, no possible) theoretical domain to which "connections might be established", or because the theory is misconceived, or both. At very least there need to be strong grounds adduced for why a theory is not so failing such as to encourage to the former of these options. The strongest grounds that Carr provides, and perhaps could provide given the nature of an autonomy claim, is the heuristic fertility of competence orientated linguistic theories, but, as we have argued, that in itself is insufficient.

The crux of our observation was that, while heuristic fertility would arise out of the constructs of a theory answering to real entities, it would also result where phenomena are caught up in a species of hermeneutic circle (a skeuomorph effect) whereby the phenomena are interpreted in the terms of the conceptual apparatus applied to the task and, mistaking the means of access and representation for the thing represented, "we predicate of the thing what lies in our method of representing it."[52r] What results is an apparent licence for taking there to be a relation of correspondence between the terms and constructs (the *abstracta*) of the prefiguring calculus and real, theory-external entities (*illata*). If this skeuomorph effect is indeed the case, two firm and confirmed predictions that fall out, and suspiciously so, are, firstly, precisely the autonomy of the domain and, secondly, its abstract ontological status. These because, to take the latter first, such entities (*abstracta* masquerading as *illata*) will have the same abstract ontological status as the prefiguring calculus and, secondly, they will also be highly resistant to confirmation from (or reduction to) other theoretical domains for the simple reason that they do not exist anywhere but as projections of the calculus. Furthermore, the putative autonomy of a domain effectively deprives the theory of one source of warrant for the reality of its object of study and its constructs, and deprives the theory of one means of excising a skeuomorph effect: autonomy of a

domain precludes theories about that domain from disconfirmation by evidence from any other domain, this insulation being packaged as ineliminable. The suspicion is that this ineliminability is no more than a case of it not occurring to us to take off the glasses, of Wittgenstein's observation, through which we see whatever we look at.

Leaving these remarks to one side, once you have a domain or "world" or "realm" of objective knowledge it seems you can get to, indeed, have to, claim the interaction between it and the other worlds as a brute fact:

> Popper wants to say that our intellectual products may have an effect on, and be affected by, the physical world via psychological states. Thus, our theories may influence our physical environment in any manipulation of the physical world we carry out.[53r]

The trouble is that this brute fact of interaction seems to be no more elucidating, nor elucidated, than Platonism's reliance on intuition:[54] the Platonist is reduced to saying that we simply "see", for example, mathematical relations, the autonomist that we simply interact with, for example, linguistic objects. The point is admitted, coyly, by Carr in the guise of a research program: "exactly how interaction ... takes place is something that needs to be investigated."[55r] In short, until some explication of this interaction is provided we are left with the linguistic as an ontological dangler, unable to be integrated into an account of linguistic behaviour, as, indeed, anomalous in the same way and for the same reasons as Platonism is. The only "advantage" is that the otherness of the second and third worlds is mediated by some wholly mysterious evolutionary saltation (in parallel to a wholly mysterious form of interaction) that, by virtue of the mystery, explains nothing. Furthermore, is there not a suspicion that an account, such as would eliminate these mysteries (a possibility that can not be precluded), would also, perhaps, even probably, disconfirm the ontology and, perhaps, reconfigure the categorization articulated by theory, and so come to show that what is wrong about an autonomous realism is not so much the autonomy but the realism, for once you drop the realism you get the autonomy for free: fictions are not likely to be observable except from the perspective of the theory that projects and posits them.

4.4 Evolution: the indiscernability of psychologism and autonomism.

As we noted in respect of the relation between competence grammars and behaviours, the recourse to mysteries is the price paid for the maintenance of a realism towards competence grammars on either side of the ontological controversy. This gets replicated in respect of the issue of the evolution of the human language faculty. To negotiate similar problems Chomsky's predilection for a reductionist psychologism needs recourse to much the same unclarities resulting in a "mysticism dressed up in a biological metaphor"[56r] or, alternatively, the posing of the problems as problems for the biologist. Chomsky's general strategy is to identify the problem (i.e. that which gives rise to the incommensurability of linguistic theories with putatively adjacent/lower level domains) in our understanding of those domains: its just a matter of getting our biology/evolutionary/brain theories right and all will be well (their incommensurability with competence theories being the evidence for *their* failings, and that means all of them[57]).

According to Pinker, the consensus is that "a uniquely human language instinct seems to be incompatible with the modern Darwinian theory of evolution."[58r] The way Chomsky comes to negotiate the incompatibility is fairly well in line with Popper: he goes the way of "emergence", thereby inheriting exactly the same problems, but the reductionism (and hence the non-autonomy of the linguistic) gets preserved and smuggled in by some hand-waving in the direction of unknown physical principles. All that it has going for it is that it papers over our ignorance in a manner in tune with the prejudices of a reductive materialism while simultaneously insulating competence theories, in a cloud of unknowing, from the criticism that the fact of their incompatibility with evolutionary theory lays them open to:

> [an innate language faculty] poses a problem for the biologist, since, if true, it is an example of true "emergence" - the appearance of a qualitatively different phenomenon at a specific stage of complexity of organization.
>
> It may be that at some remote period a mutation took place that gave rise to the property of discrete infinity, perhaps for reasons that have to do with the biology of cells, to be explained in terms of properties of physical mechanisms, now unknown.... Quite possibly other aspects of its evolutionary development again reflect the

> operation of physical laws applying to a brain of a certain degree of complexity....
>
> The answers may well lie not so much in the theory of natural selection as in molecular biology.[59]

Notice that in the latter quotation the suggestion is that the human language faculty by-passes, by way of an evolutionary saltation, a Darwinian account; what we get, by way of a "chance mutation" is the human language faculty as some sort of "hopeful monster".[60r] The standard evolutionary story has it that complex systems come about through the *gradual* accumulation of random mutations that get selected for on the basis of the advantages that accrue to the organism. Certainly, it is not easy to see how the property of discrete infinity might come about gradually, nor how it might confer any selectional advantage, particularly as this seems to require that not only unactualized possible sentences give a selectional advantage, but also impossible unactualized sentences. I suppose it is a logical possibility that some other, non-evolutionary account might turn out true, but surely it is a long shot and seems to require a view of the language faculty as, in Dennett's phrase, an (again the word) "*inexplicable* gift". It is a long shot because a language faculty is as good a candidate as you are going to get for something that does confer a selectional advantage. Consequently, it is a prime candidate for explication in terms of Darwinian gradual development, rather than as something which falls out as a non-selected-for happy by-product of an enlarged brain - serendipity indeed.[61r]

The "enthymemic" account of the instantiation of the competence grammar gets to be dealt with in the same way:

> perhaps principles now unknown enter into the functioning of the human or animal minds, in which case the notion of "physical body" must be extended, as has often happened in the past, to incorporate entities and principles of hitherto unrecognized character.[62r]

What is being floated here is the notion that some promissory brain science will be able to elucidate the nature of the missing premise of the enthymeme. It is the brain sciences that need to reconfigure *their* concepts. What is not floated is the recognition that this can cut both ways and that when this has happened in the past it has often only been achieved by way of reconfiguration, and occasionally wholesale revision, of the constructs and categorization of the assimilated theoretical domain (think what happened to phlogiston).

A problem with pre-specified promissory sciences is that they do not always turn up, either, as is our observation, as you want them or, sometimes, at all. In both cases this is to undermine claims to realism for the theory that fails to be assimilated. In the former case, because the theory has simply turned out wrong; in the latter case, because it remains dangling and anomalous in respect of the hierarchy into which, given reductionist prejudices, it is supposed to be assimilated; the theory, consequently, remaining under-confirmed.

At this juncture, an ingenious move that gets you to paper over any anomalies is to invoke the notion of "epistemic boundedness": "It may be that the operative principles are not only unknown but even humanly unknowable because of limitations on our own intellectual capacities."[63] What we are encouraged to is the view that our questions and curiosities are to be categorized as either "problems" (within our capacities and soluble) or "mysteries" (outside our capacities and so insoluble). What this buys you is a plausible rationale for a restriction on the range of data that can be adduced to the task of falsifying a theory: where heuristic fertility and predictive success fail to be supplemented by data from other theoretical domains, where there is a failure to achieve a certain theory-externality, one can take cover behind this convenient and repeatedly usable cloud of unknowing. The trouble is that there is something deeply unsatisfactory about a position where the maintenance of a realist construal, whether it be from the perspective of a reductionist psychology or of an autonomism, not only trades on, but itself generates, mysteries and/or the need for the promissory. Moreover, what these mysteries achieve is an insulation of a theory from other theoretical domains, the result being something that looks very like an autonomism but, we are assured, only looks this way because we can not explain why it isn't. The converse problem for a theory construed as autonomous is that nor can we explain why it is so.

There appear to be two directions in which things could go. The first possibility is that Chomsky's and Carr's realism will turn out to be, in general terms, well-founded. In this case, the difficulties we have been discussing will be addressed by some future enquiry such as will feasibly provide an answer to the ontological question, but without requiring any other substantial revision of the postulated object of study. In this regard, Chomsky's bet is that the apparent resistance of the linguistic to an assimilation into a reductionist hierarchy is eliminable; Carr's bet is that it is ineliminable. The second possibility is that a realism is misplaced and that the reason for the difficulties that we have been canvassing is the irreality of the objects of study

of competence theories. Despite the predictive success of competence theories, they answer only to fictions. Prime reasons for suspecting, and for investigating the possibility of, such fictionality are the variations on the theme of empirical strandedness that accompany the theory and which compound the theory-internality we earlier identified: one reason why the constructs of linguistic theory are so difficult of observation from other theoretical perspectives might feasibly be that they are fictions. On the other hand, it is just this strandedness which Carr adduces as evidence for an autonomous ontological status for the linguistic. This move is, however, just as readily interpretable as not so much a solution as a means to justify the inability to provide a solution by dignifying that inability with an ontological title (compare: this drug causes sleep because of its dormative properties; the linguistic resists integration into the natural sciences because it is autonomous). In this light, one may wonder whether it might not be worth questioning the reality of that which is so resistant, for, after all, fictions would present similar problems.

To summarize Carr's autonomist position: the linguistic object of study, the one posited by Chomsky's generativist program, is affirmed (on the grounds of heuristic fertility) but provided with a different, non-mentalist, ontological interpretation. This interpretation is argued for on the basis of the observation that it makes sense of the actual practice of linguists, (which practice gets to be normative,) in their attempt to explicate this object of study; it makes sense of the "evidential distinction" between psychology and linguistics and the correlative inability of psychology to provide (dis)-confirmatory data. In brief, it makes sense of the observation that there is, arguably, no confirmed psychological content to Chomskyan linguistic theory except for the content provided by the assertion that competence grammars are psychological: a case of throwing a word at something often enough that in the end it sticks and, arguably sticks because it gets smuggled in on the back of the heuristic fertility and on the back of a prejudice that makes the psychological basis of the linguistic unnecessary of argumentation, that is, as some sort of obviousness.

Once one recognizes that it is not an obviousness and that the psychologism does not come for free, as if it necessarily came with the linguistic territory, then Carr's contentions may be viewed as constituting a challenge to the reductionist psychologist claims of the Chomskyan project; in particular, a requirement that the reductionist psychologism is provided with proper and identifiable content, such that the putative psychological status of a competence grammar is not similarly an ontological dangler by reason of being no more than an unsubstantiated addendum.

Our aim in the following is to add some further substance to our discussion so as to show that the Chomskyan project fails in this respect and indeed collapses into an autonomism. In brief, we will argue that Carr's and Katz' diagnosis is largely correct. The trouble is I hate their suggested cure because it inherits the problems we have just been canvassing of being unable to explicate how the linguistic gets into any relation with language users, and, consequently, leaves the linguistic more or less inexplicable (except by evolutionary saltation or some equally mysterious and promissory notion of supervenience). Furthermore, it leaves it inexplicable by virtue of having to invoke an ontology for linguistic objects that from the perspective of Lycan's hierarchy of nature is anomalous.

One cannot, however, simply discount the possibility that this ontological anomaly might be the case, or that the anomaly is no more than a case of unequal levels of development in the relevant domains. One must accept one's (reductionist) prejudices as being precisely that, i.e. prejudices, even if they may have some degree of argued for warrant. Consequently, our aim will be, firstly, to attempt to further substantiate Carr's observation of the contentlessness of the reductionist psychologism of Chomskyan theory (or, to define an appropriate sense for "psychological") and, secondly, to argue that the resulting theory warrants no more than an instrumentalist construal.

To make our case, it is firstly necessary to more clearly define the corollaries of an autonomous realist linguistics in respect of what objects it takes to be real, i.e. to be quantified over, this in relation to the formal systems that are applied to in theory construction and which articulate the theoretical claims. In brief, we aim to show that the theoretical magnitudes of an autonomist theory (what is taken to be real of the independently existing object of study) cannot be differentiated from the representational constructs, the *abstracta*, of the calculus that articulates the theory. The result is that the autonomist is unable to make a choice between extensionally equivalent grammars in respect of any principles other than the purely methodological and theory-internal. The psychologist claim is that psychological facts of the matter provide such other principles and so truth-content to a choice between grammars. It is on condition that this claim is substantiated that a psychologism can be contentfully distinguished from an autonomism. We aim to show that it cannot be so distinguished and so, to that extent, Carr's autonomist contentions get to be substantiated. As we have suggested, this is not an altogether happy result for it leaves us, firstly, without the realization of the third strand of the Chomskyan project, i.e. an explanation of the linguistic, and, secondly, the reliance on purely methodological and theory-internal

considerations in respect of the choice between grammars leaves us without the realization of a contentful realism, for all we have is a "heuristic hocus-pocus".[64r]

5.0 Autonomism's and Platonism's existential commitments.

A primary distinction between a realist and an instrumentalist construal of a theory are the differing degrees of existential commitment that are involved. In the case of the former, there is an explicit commitment to the theoretical constructs of a theory: they are taken as real and existent entities rather than productive and predictive fictions. To revert to a previous illustration: once one takes the Ptolemaic model as no more than an instrumental device for generating accurate predictions of planetary motion, then one drops the existential commitment to certain elements of the model that afford those successful predictions: while one remains committed to the existence of the planetary bodies, one no longer expects, for instance, that planetary epicycles are to be discovered in nature outside of the conceptual scheme which allows one talk of the behaviour of the planetary bodies. In this case, these epicycles articulated by the conceptual scheme are to be reconstrued as "fictions in an action-predicting calculus", i.e. as *abstracta* - "calculation-bound entities or logical constructs" whose existence is only internal to the conceptual apparatus that provides the model,[65r] these being in contradistinction to entities that are posited as real and existent external to the conceptual apparatus, i.e. *illata*. Conversely, the realist construal of the theory would require just this theory-external commitment to these epicycles.

In the case of deductively formulated theory, where the theory articulated by a calculus is to be taken realistically, it is, as we have argued, a requirement, firstly, that the terms and properties of the calculus correspond, at some level of discrimination, to real elements and properties of the object modelled, and, if the theory is to be testable, that it is specified what that level of discrimination is: this amounts to a requirement that one distinguishes between what of the calculus one takes to be *abstractum* (or "representational construct") and what *illatum* (or "theoretical magnitude"). In brief, the requirement is that "the theory that accompanies the notation must tell us which aspects are intended to be empirically significant",[66r] that is to say, what the theory is existentially committed to and, if these claims are to be given warrant, that the postulated constructs are able to be credited with an existence independent of the formal apparatus that is the manner of their postulation.

It is, perhaps, a little perplexing and counter-intuitive to discover that one of the corollaries of autonomism/Platonism is that the only things it both need be and can be cogently committed to are the members of the set of sentence types (or, if we accept Postal's and Langendoen's argument (see footnote 27), members of a mega-collection of sentence types). Beyond successfully generating all the members of that set (or explicitly characterizing the members of the mega-collection) there are no further facts of the matter as regards, for example, the internal structure of sentences; there are no truth-issues involved in respect of any decision between two extensionally equivalent grammars.[67] To see how this comes about we need to rehearse a little intellectual history.[68r]

For Bloomfield a language is constituted by "the totality of utterances that can be made in a speech community",[69r] the result was a reliance on corpora of collected utterances and a view of grammars as taxonomic characterizations of those corpora. Chomsky showed that such a procedure was inadequate to the need to capture the full distributional structure of a language, i.e. to allow for a characterization of those sentences that are in the language but are outside of any particular corpus.[70r] In other words, one has to do justice not only to the actualized sentences of a language, but also to the unactualized possible sentences. Once one recognizes this need then one must, so the argument goes, also be committed to one's linguistic theory quantifying over abstract entities for the simple reason that unactualized possibilities cannot be construed as other than abstract: "unrealized entities have to be construed as universals".[71r] The upshot is that linguistic theories are concerned with such universals, standardly understood as sentence types. By implication, the relation between these objects and the products of linguistic behaviour is that of a relation between a type and its tokening.

Amongst the properties of types, by definition abstract entities, are their lack of spatio-temporal qualities, they have neither duration nor occurrence and, as a corollary, cannot be properly said to have parts: they are discrete and indivisible and so with no internal structure. For this reason "it is hopeless to try to assemble them into classes",[72r] and hopeless in respect of, for example, their internal structural properties, for the simple reason that they do not have any. The consequence is that any two grammars that achieve the same coverage, and so are extensionally equivalent, differ only in respect of their notational variation, their differing economies of information, but these differences do not constitute truth-issues. The extent of the existential commitment of a linguistic theory is restricted to just the set of sentences of a language; the manner of generating those sentences, which manner reflects the

properties of the calculus that is applied to the task, has a purely instrumental status. The result is that there is nothing to choose, in respect of truth, between a grammar that simply attempts to list the members of the set of sentences and one which generates the same set through some, or any, set of rules. Fodor makes the same observation:

> strictly speaking, the Platonist [/autonomist] has no use for distinguishing among grammars in respect of *truth* at all, so long as they make the same predictions about the speaker/hearer's intuitions.[73r]

A further corollary is that there is no sense to the notion of a sentence *qua* type having constituent parts: abstract objects are simply not the sort of things that have parts. For example, "The cat bit the rat", when considered as a type, cannot be properly said to contain the VP "bit the rat", nor constitutively contain any of the individual lexical items as parts. Certainly, each lexical item can be said to exist, as can the constituents, but they do not exist *in* the sentence *qua* type, *qua* abstract object, but only independently as distinct types. On the one hand, one might observe that this leads to there being rather a lot of such entities, none of which has any mereological relation with any other, i.e. no relation of being a constitutive part of any other whole. On the other hand, and extremely bizarrely, we get a set of linguistic objects, sentences and constituents (*the* linguistic objects - they are, after all, the objects of study), that lack most of the mereological properties that linguists are interested in and consistently talk about. What results is consensus about what we are studying (sentence types) and then, in this light, an absurd counter-consensus to talk about something else: the internal structure of sentences, which internal structure is precisely what sentence types do not have. The only things that have such internal parts are sentences considered as structured sets of tokens, but did we not just agree that tokens, i.e. corpora of utterances, cannot do service as our proper objects of study?[74r] This leads to an uncomfortable quandary. On the one hand, if one insists in construing the linguistic domain as constituted by a set of sentence types, then this is to undermine any realism towards the terms and constructs of linguistic discourse where that discourse imagines itself to address the internal structure of sentences, for such internal structure vis-à-vis sentence types can be no more than a *fictional* projection of structure. I suppose one can take this direction if one really wants to, only it is hard to see why anyone would want to. For one thing, linguistic enquiry would have virtually nothing to say about the everyday phenomenology of the

linguistic, the actual products of linguistic behaviour which, being spatio-temporal, do have internal parts. This would leave it open for a different linguistics to be instituted, one that does address this area of study. On the other hand, and in the light of this, one might ask:

> why should linguists bother with types (...) at all, when all the linguistic properties with which they are most crucially concerned are elsewhere - always and only in utterances (the supposed tokens)?[75]

The implication is that it is the aforementioned and promissory "different linguistics" that is the proper, prior, and, let's face it, interesting area of enquiry and that linguistic realities are restricted to just these physical, actual and actualized tokens. If this is the case (despite the previously canvassed inadequacies of this approach), then, as Burton-Roberts observes, linguists' discourse, in quantifying over types (i.e. abstract entities that are non-occurring and non-physical), fails to refer to the real, calculus-external linguistic entities (physically occurring utterances); the referents of the linguist's discourse are no more than "calculation-bound" *abstracta*:

> if the linguistic is abstract (not physical), it is so only in the sense of being derived by abstraction (generalisation, idealisation, classification) over the 'real linguistic data', namely physical utterance phenomena; ... what is abstract in linguistics is not of-language, but merely of-and-for-the-linguist [of-and-for-the-calculus], having a (merely) theoretical [instrumental] utility for the linguist in his efforts to come to grips with the real object of his enquiry, ...[76]

In sum, the fact that the linguistic is in excess of any set of actualized sentences, (hence, requiring of a linguistics that it be concerned with abstract objects,) is at odds with a second fact: that structural properties cannot be cogently predicated of these abstract objects. If we address the latter fact by reverting to tokens, we do not get the requisite generality and the abstraction of the constructs of our discourse is "merely of-and-for-the-linguist"; if we address the former fact, then the truth-issue is no more (and, of course, no less) than one of devising a grammar that correctly generates the members of the set of sentences, but there are no truth-issues beyond this, in respect of, in Chomsky's phrase, "the principles used", i.e. the particular economy of information of which a grammar makes use. The particular economy of

information is also "merely of-and-for-the-linguist", i.e. it has a purely instrumental role.

What these remarks suggest is, firstly, that there is something wrong somewhere, and seemingly the something wrong has to do with the identification of the object of study as a set of sentence types, and, secondly, the question as to whether these somewhat unpalatable conclusions can be avoided, preferably without the need for wholesale, foundational revision.

Chomsky's proffered solution (which we will elaborate more fully below in chapter VI) is to reconfigure the object of study: rather than the linguist's concern being directly with the set of sentences in a language (i.e. the E-language, a set of abstract types - for it is the taking of this as the primary object of study which, in seemingly precluding a reductionist psychologism, underpins autonomist/Platonist approaches;) instead, what the linguist is to address is the nature of the I-language, the mind-/brain-internally realized set of rules (or principles and parameters). It is this set of rules, with their informational content - ultimately neurological structures - to which the linguist's grammar is required to correspond. These rules are, putatively, the linguistic realities, they are what a linguistic theory is to be true of, they are the facts of the matter and facts that are involved in the aetiology of linguistic behaviours, amongst which behaviours are speaker intuitions. It is this which makes sense (in respect of truth) of any choice between extensionally equivalent grammars; the putative mental representations and their economy of information constitute the "principles" which provide the content to any decision between grammars, they are what our theories are in attempt to get in correspondence with, that is, to be true of.[77] What we also get for free is a rationale for the taking of intuitions as data, a rationale which is a requirement - we need to know why the data are appropriate and (more or less) veridical data - and which rationale a Platonism precludes, needing resort to the mysteries of "intuition" to paper over the discrepancy. In this way the realist construal of a grammar is dependent on the mind/brain reality of these informational structures; it is these which are "psychologically real" and not the set of sentences, for such sets, Chomsky tells us, "are not in the mind/brain".[78r]

Such is the proposal which distinguishes the Chomskyan generativist enterprise from autonomist/Platonist construals. It is, however, one thing to propose, another to dispose. We take up, in chapter VI, the issue of whether Chomsky's I-language diagnosis is sufficient cure. That aside, if we are to be convinced, then we need grounds for belief by being provided with properly achieved content to the I-

language. The Chomskyan view, in contending that there are truth-issues - mind/brain-internal truth-issues - involved in respect of a choice between extensionally equivalent grammars, needs to be able to provide some proper, truth-relevant criteria for allowing a decision between such grammars. If such cannot be provided, then the psychological realism of the Chomskyan position fails to substantiate the content of "psychological" and becomes indistinguishable from a Platonism/autonomism. But then, as we are attempting to suggest, what is wrong about a Platonism/autonomism is its claim to be answering to any facts of the matter that exist external to the conceptual apparatus which articulates theory. It is not so much or only the autonomy that we are objecting to, but the realism.

For the Platonist/autonomist the data are restricted to intuitions because the Platonist/autonomist position insists on, because the coherence of the position requires, (in explicit contradiction of the Chomskyan view (see, for example, N. Chomsky, 1986, pg. 39)) the irrelevance of any other data. This seems to raise a pair of problems. Firstly, to reprise our previous remarks, we might want to be told a story, preferably one that does not take refuge in mysteries, as to what grounds there are for taking intuitions as *the* data; for if the linguistic is not to be granted a psychological locus then we forfeit Chomsky's rationale for accepting intuitions as relevant, i.e. they are relevant because they reflect a speaker's tacit competence and reflect it because that competence has a role in the aetiology of the intuition. The point is emphasized by Fodor (his italics): "an adequate linguistics *should explain why it is that the intuitions of speaker/hearers constitute data relevant to the confirmation of grammars*."[79r]

As we noted, Katz makes an attempt at an account but, while it gets him what he needs, the relevance of intuitions, this is achieved at the cost of credibility. Likewise, Carr has to wave vaguely at the "fact" of "interaction" and hope that we do not ask for an explanation.

The second problem, and one that infects Chomsky's position, (again an issue taken up in chapter VI) is that of having to explain how it is that our intuitions are fallible. The anomaly is that, in the case of certain sentences, (e.g. multiple centre-embeddings, which are grammatical but unacceptable,) we seem to have to flout the law of excluded middle by having to claim that what we know, our tacit competence, (which is identified with our ability to have intuitions; it is the knowledge resource we are supposed to access,) we simultaneously do not know (so our intuitions, where they go wrong, are not identified with our tacit competence). In Carr's words, Chomsky's

problem is that of "how it can be that our cognitive resources allow for such structures and cannot cope with them at the same time."[80] Carr takes this as grist to the autonomist's mill: the fact that such sentences are licensed, but exceed our cognitive abilities is "more easily interpretable" if one makes "a distinction between linguistic facts and cognitive psychological facts." The problem, though, is that if one makes this distinction, it is hard to see why intuitions should be taken as trustworthy data at all, and if they are not trustworthy (as they may not be, given the consensus that they are on occasions fallible, and in which case, how do we know when they are trustworthy?), then just what are the constraints on the licensing of sentence types by theories? Once one removes the psychological locus for the linguistic, then while one gets to justify actual practice in which data other than intuitions are effectively irrelevant, unfortunately, one also undermines the rationale for taking intuitions as relevant data in the first place, and, along with this, it seems one is absolved from having to answer to mere actuality: it's positively Liberty Hall.

Carr's observation is only grist to his mill if we *know* that such (unacceptable but grammatical) sentences are proper objects of the linguist's concern, that is as licensed sentences of a language, as real linguistic objects. (Where "linguist" is someone who is interested in natural, as opposed to artificial, languages.) But, how do we know this, just what is the authority for such sentences being members of the set of sentences in a natural language? Just what queer facts are these that we know but which are "not cognitive"? There are no possible data, neither as actualized tokens, nor, when the linguist artificially constructs such sentences, are there any speaker judgements of their acceptability. The only authority is the calculus/grammar (a non-cognitive object? What sort of minds do linguists have?) which generates them along with the rest of the infinite set. But, in this case, how are these unactualized and unactualizable objects, to which the calculus is supposed to be in a modelling relation, (that relation is, after all, what linguistic enquiry is in the business of testing,) to be distinguished as real members of the set of sentences of a natural language as opposed to real members of the set of sentences of an artificial language? They are simply unidentifiable except as objects that are required to exist so as to interpret the structures generated by the calculus, having no discernible existence except as "*abstracta* - calculation-bound entities or logical constructs". But this is not to test the modelling/generative ability of some calculus, but it is simply to assume it. (And it is also to assume, as if on some grounds of conceptual necessity, that there is "no important theoretical difference between natural languages and the artificial languages of logicians."[81]) And in assuming it, and quantifying over the set of objects that

interprets the generated structures, i.e. which supplies its model vis-á-vis syntactic objects (this is how we get to say a language consists of an always and already given set of sentence types), it is not just a case of our conceptual spectacles tending to structure how, and what, we see in the world, but they also invent it.[82]

Furthermore, this problem does not just infect the relation between the types generated by the calculus and their impossible tokenings, but, given the incommensurability of the properties of types and any actual utterances, then we get a peculiar bifurcation such that the actual linguistic, i.e. tokens, the products of linguistic behaviour, what would appear to have some claim to constitute a pre-theoretical given and first order phenomenology of the linguistic, remain distinct of, and unable to be assimilated with, what is being taken as the proper object of study: east is east and west is west and never the twain - the linguistic and actuality - shall meet. To reprise Carr's comments above, it would seem that this situation is "more easily interpretable" if one makes a distinction between the linguist's object of study with its "facts", to which linguists alone have privileged access, and the actual, real world linguistic facts. Only do not the former look like, for how can we distinguish them from, phantasms?

The result is that an autonomous/Platonist linguistics begins to look indistinguishable from the study of formal calculi *simpliciter*, with the object of study (an always and already set of sentences, putatively linguistic reality) looking suspiciously like (because indistinguishable from) an artefact constructed out of the formal properties of the mathematical/logical apparatus that has, as we noted, beyond the role of generating the right set of sentences, a purely instrumental role. But it is not just the economy of information of a grammar, that carries no truth claim, the calculus-external, actual-world fictionality also extends to members of this set of types that are in the extension of the grammar but are not actualizable or recognizable as acceptable sentences. With this losing of contact, the object of study begins to look like a skeuomorph, nothing corresponds to it except the abstract generative capacity of the formal calculus, which calculus, of course, has one ineliminable modelling relation: it ineliminably models itself (see footnote 82). Our concern will be with assessing the extent to which Chomsky's psychologism succeeds in negotiating these problems and so distinguishing its object of study from the phantasmic.

6.0 Choosing a grammar.

Leaving the above issues to one side, although Platonism/autonomism results in the situation where extensionally equivalent grammars cannot be decided between on empirical grounds (beyond the apparently negotiable criterion of answering to speaker intuitions), this does not vitiate the desirability of choosing between competing formulations of the grammar in respect of other criteria. The relevant criteria here are those of simplicity, economy and elegance, the standard set of considerations that are applied to in construction of scientific theories.

But just what is at stake in invoking these criteria in respect of a special science, as opposed to a natural science? The answer would appear to be quite proper practical considerations regarding ease of use and applicability, these being particularly relevant in respect of possible computational applications. Alternatively, but amounting to the same, one might prefer one formulation over another simply on aesthetic grounds: one formulation is able to capture useful generalizations in a neat and systematic way whereas another does not. Crucially, these considerations refer to the calculi employed in theory construction as vehicles of representation for that theory and they are criteria relevant to the assessment of those calculi in respect of their comparative efficiency as vehicles of representation, not in respect of the empirical claims of the theory that is articulated through some calculus. Recall, our concern is with examining the comparative merits of extensionally equivalent frameworks that all express the same theory, that is all predict the same set of sentences and which sentences are the sum of the existential commitment of these frameworks.

The situation is akin to a choice between different ways in which we might represent the natural numbers in mathematics or how we choose to represent first order logics. To take the former to illustrate: two notations may both express the same theory, i.e. have the same model, and so quantify over the same entities, in this case the set of natural numbers. This set may be represented by either arabic or roman numerals with no different truth claims being involved in respect of either: they are extensionally equivalent and have the same existential commitments, i.e. make reference to (or, in model-theoretic terms, are interpreted by) exactly the same set of entities. However, we standardly use the former for the reason that the arabic numerals admit of much easier implementation in the algorithmic procedures that we use to perform the mathematical functions (e.g. try multiplying CCLXXIII by CXLIV without transposing into arabic numerals). The notion of ease of use is, however,

relative to the applications one has in mind: arabic numerals are not used in computational contexts, rather the binary system, again because of its ease of implementation, this time in respect of the basic properties of computational hardware. Certain indirect truth-issues do, however, arise out of the choice of notation. For example, the development of mathematics in Europe was greatly facilitated by the introduction of the arabic numerals. This is not because they are any more true, but because they are easier of manipulation; indeed, it is the properties of the notation that make available the algorithms that we use in, for example, multiplication and division.[83r] Choice of notation has a heuristic value. There are parallels in linguistics. Tree-diagrams and bracketing notation are taken to be equivalent. It is arguable, however, that it is the use of the former that makes available such notions as government and c-command - relations which are simply not salient, even if implicit, in bracketing notation.[84r]

These observations are not confined to the special sciences. Reverting to astronomy again, Pylyshyn points out that:

> Apart from issues of parsimony and generality, we do not argue about which of two equally predictive formulations of classical mechanics provides the correct explanation of planetary motion .... The question simply does not arise in mechanics because no ontological claims are made about the notation in which the equations are cast.[85r]

There are two points to be underlined here, one for future reference and the other in respect of the content of Chomsky's notion of I-language. Firstly, a theory's truth does not correlate, necessarily, with some measure of simplicity, as if, given two equally predictively successful theories/grammars, then the simpler one is, *ipso facto*, more likely to be true. Certainly, all thing being equal, it is to be preferred *as a hypothesis* on grounds relevant to the methodology of theory construction, but on methodological grounds alone. (A hypothesis, even the most methodologically sound hypothesis, cannot be taken to correspond to what is the case. If we could rely on our methodology of theory construction, science would be a purely armchair enterprise.) It is a brute fact that, in certain cases, and in some fields more than others - notably and very relevantly biology - it is the messier theory that is the more accurate.[86] The moral of the story is that preference for a theory on the grounds of considerations of

simplicity - which considerations are precisely theory-internal considerations - can not double as preference for a theory on the grounds of empirical truth; the fact of a theory's internal simplicity is not itself warrant for a realist construal of that theory.

Secondly, Pylyshyn's remarks provide a commentary on Chomsky's positing of the I-language as the object of study, and on how this thesis relates to the two general observations we have made regarding a Platonist/autonomist perspective: firstly, the purely instrumental status of the economy of information of a grammar and, secondly, the inability to maintain contact between actual linguistic objects and events and the abstract objects that interpret the set of strings generated by a calculus.

Although this will be finessed, essentially what the I-language perspective requires so as to achieve content is existential commitment to entities and relations that correspond as the referents of the notation of a grammar such that there is, putatively, a substantive argument over extensionally equivalent formulations.[87] The terms and relations of a calculus are not simply instrumental means to a characterization of the phenomena, not simply representational constructs (*abstracta*), but theoretical magnitudes (*illata*): the linguist's grammar corresponds to some psychological structure that represents the same informational economy as that grammar. It is the internally represented grammar that constitutes the object of study, and it is the properties of that mentally represented grammar that constitute the facts of the matter, i.e. what one is to be realist about, this as opposed to sets of sentences. In making this move one is addressing what is, perhaps, the primary inadequacy of a Platonist/ autonomist linguistics, that is the failure to address the question of the relation between linguistic objects and their instantiation in linguistic behaviour through those linguistic objects being in some relation with individual speaker/hearers of natural languages. In short, one needs some account from the perspective of individual psychology, this because it at least makes sense of the use of intuitions as the primary source of data, and this needs to be made sense of to justify anyone in taking the enquiry, with its methodology, half seriously.

On the other hand, we want, if not need, this perspective because the question of how individuals come to be related to the linguistic does not go away simply by ignoring it. It is equivalent to the question as to why natural language is as it is; it is the attempt to answer this question that makes the enquiry more than a taxonomy and is, arguably, what makes the field interesting. Fodor makes fairly well the same point:

> Suppose that we grant the Platonist proprietary use of the term 'linguistics'. So, by stipulation, linguistics is part of mathematics. But then, just down the road there

> must be another science *just like linguistics* except that it *does* care about empirical truth because it cares about how the mind works.[88r]

Going down the road and taking a perspective from individual psychology constitutes Chomsky's wager: that should such a psychological linguistics be achieved, then that would exhaust what is to be explained in respect of the linguistic:

> Knowing everything about the mind/brain, a Platonist would argue, we still have no basis for determining the truths of arithmetic or set theory, but there is not the slightest reason to suppose that there are truths of language that would still escape our grasp.[89r]

The wager is, in short, that "language has no objective existence apart from its mental representation."[90r] It is this hypothesis which offers to solve the anomalous and ineliminable *sui generity* that is argued for by the Platonists on the basis of the nature of the object of study postulated by Chomsky's generativist enterprise. If it can be shown that this anomalousness cannot be excised and the psychological claim not achieve proper content, then, perhaps, despite its own problems, we must take the Platonist/autonomist option. But that would leave, down the road, the same promissory science Fodor alludes to, for which foundations have to be set, or, alternatively an option on reassessing the status and propriety of the object of study that is hypothesized by the Chomskyan project.

These remarks set an agenda: whether it is possible to distinguish between Chomsky's psychologism and an autonomism on the basis of achieved content for the psychological claim.

Footnotes.

[1] This is not to require that such a statement is a precondition of theory development. Compare Quine's remarks (chapter 2, footnote 26).

[2] We use the term "reductionist" here in respect of Chomsky's claim that linguistics is a sub-branch of psychology and ultimately of biology (N. Chomsky, 1986). The same position appears to be taken in more recent writings where the human language faculty is described as a "biological system" (N. Chomsky, 1995). This reductionism is not of the eliminative sort (i.e. an eliminativist would claim that the constructs of some discourse are irreal, even if predictively accurate, and are to be wholly replaced by the constructs of some other more basic level discourse). Rather, I take it that Chomsky holds the view that some completed neuro-science will be commensurable with the constructs of linguistic theory (in a manner similar to which the gas laws fall out from a more basic level molecular theory, (see below, pg. 98)).

[3] In a similar way, what undermined confidence in the Galilean prejudice that the world is maths writ large was, in the case of mathematics, the discovery/invention of non-Euclidean geometries and, in logic, of higher-valued logics. (See our discussion in chapter I, 2.1 and J. Barrow, (1993, pgs. 8 - 20)).

[4] D. Marr (1982).

[5] J. Katz (1981, pg. 180).

[6] C. Pollard and I. Sag, (1994, pg. 14). Similar agnosticism is expressed by Gazdar *et al.* (1983, pg. 5).

[7] S. Blackburn, (1994, pg. 84).

[8] P. Carr (1990). Carr's species of autonomism is founded on a Popperian (1972) view of "objective knowledge" which is argued to be "inter-subjective" and not dependent on, and so autonomous of, individual psychologies. For more on this, see below.

[9] An example of the consensus view is given by Pollard and Sag: "one thing that [language] certainly does not consist of is individual linguistic events or utterance tokens, ... Instead, what is known in common, ..., is the system of linguistic types." (C. Pollard and I. Sag, 1994, pg. 14). See also Bromberger, (1989). Indeed, it is difficult to find any dissenting voices, see, however, Sampson, (G. Sampson, 1976). For an account of some of the uncomfortable corollaries of taking the relation between linguistic objects and the products of linguistic behaviour as a type/token relation, see Burton-Roberts, (1994, pgs. 186 - 189). Indeed, just such uncomfortable corollaries, we will argue, are the linguistic counterpart of quantum mechanics' wave-particle duality.

The type/token distinction was first given an explicit formulation by Peirce:

> There will ordinarily be about twenty "the"s on a page, and of course they count as twenty words. In another sense of the word "word", however, there is but one "the" in the English language; ... it is impossible that this word should lie visibly on a page or be heard in any voice. (C. S. Peirce, 1958, pg 423)

"A word in this [latter] sense is not a physical object, not a dribble of ink or an incision in granite, but an abstract object. ... Words in the first sense have come to be called *tokens*; words in the second sense are called *types*." (W. V. O. Quine, 1987, pg 217.)

For a survey of type-token thinking in linguistics, see C. Hutton (1990).

[10] Note that taking the objects of study of linguistics to be *sui generis*, as is the consensus, is not to claim, in the same motion, that the linguistic is irreducible and so ontologically autonomous. Rather, what one is claiming is that there is a discrete, nomological domain which is characterizable in terms of laws that are peculiar to that domain. These laws themselves may be consequences of more general laws. This is the case when the former domain is reducible to the latter. For more on this, see below.

[11] We will take "special science" (Fodor's phrase) to refer to any area of discourse that resists reduction to any other discourse, not necessarily on the basis of an ontological distinction.

[12] The phrase is borrowed from Carr (P. Carr, 1990, pg. 35).

[13] P. S. Churchland and T. Sejnowski, (1990, pg. 229).

[14] It might be properly objected that this is stretching the notion of causality somewhat, however, the point is to distinguish and refer to two senses of "explanation", it is not intended as an account of the nature of causation.

[15] N. Chomsky, (1986, pg. 23). Perhaps we have come, out of familiarity, to forget to notice the boldness - in the best Popperian tradition - of this hypothesis. It is, after all, analogous to proposing that the planets have a body of internally represented propositional knowledge to which they in some sense refer, and which is (literally) causally implicated in their behaviour. A hypothesis, I imagine, that would not find (and would never have found) many takers. But then, planets are not the sorts of thing we would credit with minds nor knowledge and, in the human linguistic case, it is this knowledge which is what is accounted for by a grammar. However, what our illustration demonstrates is that a specification, through a grammar, of what one knows when one knows a language, in effect a specification of the laws of the domain, is not by itself a causal explanation, but is, rather, what is required to be explained.

[16] W. Lycan, (1990, pg. 78).

[17] The term originates in the work of the nineteenth century philosopher W. Whewell (1840). The man seemingly had a talent for coining neologisms being also responsible for the term "scientist".

[18] Compare S. Bromberger (1989).

[19] J. A. Fodor, (1985, pg. 157).

[20] S. Bromberger, (1989, pg. 85). Bromberger's argument is, in fact, from a slightly different direction: his suggestion is that we need a psychological locus for the linguistic such as to provide something for linguistic laws to be determinately true of, otherwise (this is my interpretative spin) there is nothing to distinguish the laws being about anything other than fictions.

[21] N. Chomsky, (1986, pg. 39).

[22] The case of Williams Syndrome is particularly relevant. The characteristics of the condition include both clearly physical and cognitive elements. Physically, sufferers share an "elfin-like" facial physiognomy and a susceptibility to certain forms of heart condition; cognitively the condition is marked by a low IQ, with particular problems in spatial and quantitative reasoning. However, sufferers are reported to have above average social skills and a corresponding linguistic facility.

[23] W. Lycan, (1990, pg. 79).

[24] See M. Gopnik (1994) and M. Gopnik and M. Crago (1991). The conclusion the latter draw is that "it is not unreasonable to entertain an interim hypothesis that a single dominant gene controls for those mechanisms that result in a child's ability to construct the paradigms that constitute morphology." (pg. 47). For an overview of research in these areas, see Pinker (1994, chapter 10).

[25] This is not a novel position. See, for example, P. Carr (1990, Chapters 1 and 2). The same conclusion is reached on different grounds to Carr's by Katz (J. Katz, 1981 and 1996) and Langendoen and Postal (D. Langendoen and P. Postal, 1984).

[26] J. A. Fodor, (1985).

[27] The former of these reflects a central area of tension in Chomsky's thinking, that of how to maintain a relation, primarily a causal/explanatory one, between the "determinate neural structures" that instantiate the I-language and the abstraction of the sentence types that the I-language generates.

An instance of the latter argument is Langendoen's and Postal's (D. Langendoen and P. Postal, 1984). It is akin to Gödel's Incompleteness Theorem (compare Yourgrau (1989)) and is used by them in much the same manner as Penrose (1991) applies the latter to "prove" the inadequacy of the notion of the mind as a computer, i.e. in both the linguistic and the mathematical case there are sentences/statements that are beyond the capacities of generative grammars/computers. Furthermore, in much the same way that Gödel's theorem grounds his Platonism, it also grounds theirs.

In very brief, their point is that the collection of sentences of a natural language, in view of the facts of co-ordination, is not given by any number, finite or transfinite. Natural languages, hence, exceed the capabilities of any generative grammar. Correlatively, most sentences of a natural language cannot be psychological entities, they exceed the capacity of a finite brain. Although Chomsky maintains that sets of sentences are not the direct objects of study (1986, pg. 34) (rather it is the I-language, a finite set of principles and parameters - possibly this reformulation is in response to their argument), the relation between the I-language and the sentences of a language becomes problematic because there simply can be no psychological relation between that I-language and most of the sentences of a language, i.e. those sentences, the vast majority, that are too big to have a mental representation.

For other arguments to the same effect, i.e. the nature of linguistic objects is incompatible with their having a psychological ontological status, see Katz and Postal (1991). These issues will be taken up again below (in chapter VI).

[28] One might wonder how a speaker hearer gets into some appropriate relation with these linguistic objects such as to have intuitions at all, let alone intuitions that are to be trusted, but we will gloss over the issue for the moment.

[29] P. Carr (1990, pg. 54).

[30] Arguably, this is why there is a comparative lack of interest in the ontological question: nothing practical hangs on it so one might as well ignore it. This should not be a source of comfort: the downside is that all that this suggests is that, quite literally, linguists do not properly know what they are talking about.

[31] P. Carr (1990, pg. 36).

[32] *Ibid.* pg. 35.

[33] *Ibid.* pg. 45.

[34] W. V. O. Quine, 1960.

The point, of course, is that there is no *a priori* metaphysical, as opposed to methodological, virtue in either richness or economy; the point is to get it right and that means having as many things in one's ontological catalogue as there are types of thing in the world, neither more nor less. This observation may be taken as a corrective to a common shibboleth-like invocation of Occam's Razor as some sort of metaphysical principle of maximal economy. This would be true only if there were grounds for believing the world to be designed on maximally economic lines. This could be the case, but it could only be shown to be so *a posteriori*. On the other hand, to suppose that it is the case is a species of Galileanism, the world being the creation of some perfect mathematician valuing elegance and economy above all else. A proper use of Occam's Razor is in respect of practicality and methodology in theory construction. Given some phenomenon to be explained, one constructs a theory that has as few explanatory constructs as are required to do the explanatory job, so avoiding redundancy, making fewer guesses and so making the theory easier to test. *Then one tests it* and it is the testing of the hypothesis, not its conforming to a methodology of hypothesis formation and theory development, that is the arbiter.

[35] W. V. O. Quine, from "On What There Is", in W. V. O. Quine (1961). Quine's target is Meinong's universe of subsistent entities.

[36] P. Benacerraf and H. Putnam (1983), cited in J. Barrow (1992, pg. 272).

[37] J. Katz (1981, pgs. 201 ff.). Katz' position here, and the problem he is in attempt to address, has more than a passing resemblance to Burton-Roberts' (1994) "representational conjecture". This resemblance, despite the latter's avowed "cognitivism", is perhaps not too surprising as they are both trying to negotiate the same problem: that of how to preserve a realism vis-á-vis the standardly accepted object of study and get it to be related to actualizations of the linguistic in behaviours. We address Burton-Robert's hypothesis in Chapter VI.

[38] The "may not" clause is to cater for the cases where our acceptability judgements are mistaken. It rather leaves the question begging as to how the linguist can know that they are mistaken.

[39] J. A. Fodor, (1985, pg. 160).

[40] "Representations" in a slightly Pickwickian sense in that they do not represent anything external to the representations; they represent themselves, as it were. Imagine a piece of non-figurative abstract art and the appropriate sense of "representation" that is pertinent to it.

[41] Chomsky has himself suggested the similarity: "One could perhaps take the intuitionist view of mathematics as being not unlike the linguistic view of grammar." (1982, pg. 16). Where this analogy fails (between the Chomskyan conception of the linguistic and the intuitionist's conception of the mathematical) is in the intuitionist's emphasis on the construction process, its existence in time and its algorithms, the mathematical correlate of linguistic performance. We return to these issues in chapter VI.

[42] The first being the practical restrictions on data mentioned previously; i.e. generativism's empirical strandedness.

[43] J. A. Fodor (1983, pg. 9).

[44] See K. Popper and J. Eccles, (1977, pgs. 16 ff. and 36 - 50). There are more than passing similarities with Frege's "three realms" (G. Frege, 1967). Frege's classification is itself a symptom of his Platonism in respect of numbers, sets and propositions. It is interesting to note how the Fregean scheme works out in respect of the linguistic: amongst physical things are counted written or uttered sentences (i.e. tokens); in the second realm we have the mental "ideas" that accompany (process?) the tokens, and the third realm's denizens include the propositions (and, presumably, the syntactic structures) expressed or, perhaps more accurately, revealed: they are after all entities that exist always and already, independently of physical and mental events.

It is also interesting to note a curious parallel between the Fregean and Popperian trinities and that of mainstream cognitivism with its (after Marr (1982)) three levels of cognitive description: level one, the function computed (cf world/realm three), two, the algorithm, (the psychological construed as intermediary process) and three, the hardware (the physical).

Hersh (1998, pg. 220) reports that notions of autonomy of "levels of reality" and "emergent evolution" are not new and he points, as does Blitz (1992), to R. Sellars' *Critical Realism* (1916) as a precursor.

[45] "A property of a complex system is said to be 'emergent' just in case, although it arises out of the properties and relations characterizing its simpler constituents, it is neither predictable from, nor reducible to, these lower-level characteristics." (in T. Honderich (ed.), 1995, pg. 224). A putative and, I understand, not uncontroversial example is the transparency of water which, it is claimed, is not predictable from the properties of, and the laws relevant to, the hydrogen and oxygen atoms of which it is composed.

[46] K. Popper and J. Eccles (1977, pg. 16). Cited in Carr, (1991, pg. 40).

[47] *Ibid.* pg. 16.

[48] The phrase is Churchland's (P. M. Churchland, 1990, pg. 219).

[49] S. Blackburn, (1994, pg. 368).

[50] W. Bechtel, (1990, pg. 266).

[51] As examples of categories that have undergone such reconfiguration and revision, Churchland and Sejnowski cite impetus, caloric, gene, neuron and electricity amongst others. (1990, pg. 229).

[52] L. Wittgenstein, (1958, section 104).

[53] P. Carr (1991, pg. 40).

[54] Recall Dennett's observation that citing "intuition" is little other than admitting that we do not know what is going on.

[55] P. Carr, *opus cited.* pg. 44.

[56] The phrase is Pinker's (1994, pg. 317). Compare with Ryle's phrase (he had in mind the innateness hypothesis, but the general point is the same) "clouds of biological glory."

[57] See, for example, N. Chomsky, (1972, pg. 70 and 1995, pg. 386). I suspect that biologists, evolutionary theorists and brain scientists might object to this assumption as to who is getting it wrong and quite reasonably make the same charge only going in the contrary direction. One suspects that the argument is not readily decidable one way or the other (one can argue the toss all day) as regards the claims of the brain sciences, but it seems at least a bit rich to suggest that Darwinian evolutionary theory (which surely must rate as about as explanatorily successful as theories get) is flawed or irrelevant - i.e. some other explanation is needed (see, Chomsky, 1972, pg. 98) - because it appears unable to give an account of how a competence grammar might have evolved.

[58] S. Pinker, (1994, pg. 333).

[59] The former quotation is from N. Chomsky, (1972, pg. 70), the latter N. Chomsky, (1988, pg. 170 and pg. 167), cited in D. Dennett, (1995, pg. 389) and S. Pinker (1994, pg. 355). It might be noted that much the same considerations in respect of the qualitatively different phenomenon of mind lead a recent Nobel prize-winning neurologist to propose that what has been going on is "supernatural spiritual creation" (J. Eccles, (Popper's co-author) 1989, pg. 287). It is not immediately obvious where the two approaches differ in respect of any hard explanatory content, all we have are different packagings for our ignorance.

For a discussion and rebuttal of Chomsky's agnosticism toward Darwinian evolutionary theory in respect of the human language faculty, see Dennett, (1995, pgs. 384 - 393) and Pinker, (1994, pgs. 354 - 364).

[60] The phrase is Dennett's (1995, pg. 391).

[61] For an argument for the relevance of a Darwinian account, see Pinker and Bloom, (1990). The by-product idea is canvassed by Gould as "the evolutionary reading for Chomsky's theory" (S. Gould, 1990, pg. 14).

[62] N. Chomsky, (1980, pg. 6).

[63] *Ibid.*

It is amusing to note that a similar line was taken by the Pope with whom Galileo came into conflict. We are encouraged to take, on faith, the Church's teaching because "God could - and indeed may - have brought about the observed effects 'in many ways unthinkable to our minds'" (M. Hoskin, 1997, pg. 133).

[64] C. Hutton (1993, pg. 175).

[65] The phrases are Dennett's (1981, pg. 13).

[66] Z. Pylyshyn, (1991, pg. 239).

[67] Chomsky (1986, pgs. 20 ff.), in positing his distinction between E-languages and I-languages, makes a similar point, and takes this point as indicating the inability of an E-language approach to explicate any notion of internal structure.

[68] For an elegant and concise summation of these issues, see S. Bromberger (1989, pgs. 58 - 62).

[69] L. Bloomfield, (1928) cited in N. Chomsky, (1986).

[70] N. Chomsky, (1975).

[71] W. V. O. Quine (1960, pg. 34), cited in J. Katz, (1996).

[72] *Ibid.*, pg. 34.

[73] J. A. Fodor, (1985, pg 159). Also compare Chomsky (1986, pg. 20).

[74] This observation is owing to Burton-Roberts, (1994, pg. 188). See also A. Kasher (1972, pg. 331) who notes "the difficulty for those who would like to maintain that sentences are series of smaller elements." One might come to think that a type-token distinction is more trouble than it is worth.

[75] N. Burton-Roberts, *ibid.* It is to be noted that Burton-Roberts does not advocate such an elimination of type talk.

[76] *Ibid*, (pg. 189). Again this position is not one that Burton-Roberts is party to, rather it is a position that his paper attempts to counteract.

[77] Recall Chomsky's contention (1980, pg. 11) that "substantial coverage ... is not a particularly significant result; ... [because it] is not very informative as to the correctness of the principles employed".

[78] N. Chomsky, (1980, pg. 34).

[79] J. A. Fodor, (1985, pg. 152).

[80] P. Carr, (1990, pg. 43). The standard answer is, of course, by way of the competence/performance distinction. As we illustrated previously (chapter I, section 4), perhaps this might be better termed a distinction between competence and a bottomless rag-bag of *ad hoc* manoeuvres.

[81] R. Montague (1970), cited in Gamut (1991, pg. 214). "On this point," Montague remarks, he "agree[s] with Chomsky and his associates." It is worth reminding ourselves that there is, to my knowledge at least, no ***a priori*** reason why this should be the case (except on the basis of some Galilean assumption).

[82] This quantifying over abstract objects, i.e. the syntactic structures that are generated by a grammar, is a reflex of the "natural impulse" to a referential/denotative theory of meaning: in this case it is the strings/theorems of a calculus that are interpreted by a model, supposedly some aspect of the real world, as syntactic objects. We need such a model with its objects, apparently, if a grammar is not to be just the paper-written symbols of a formalist's nominalism.

Putting things this way gives us a purchase on the notion of notational variation: two grammars are notational variants where the same model, a set of syntactic objects, interprets both. They are not notational variants just in case that they require different models. The picture gets complicated somewhat if we take on board Putnam's (1980, 1981 and 1989) argument (*mutatis mutandis) contra* a model-theoretic semantics for natural language (and hence *contra* an autonomy of syntax from semantics thesis). This argument is to the purpose of showing, with a result reminiscent of Quine's indeterminacy of translation thesis, that for any one formal system there are an indefinite number of models that can act as the interpretation of that system. The cogency of a realism depends on the model (what our notation is about) being existent independently of theory and also it being possible to answer the question, "which one?". The problem for a realism is that the question does not appear answerable. Moreover, Putnam reminds us that while some model is supposed to supply the meaning of the axioms (for our present purposes, supply the syntactic objects that are the syntactic "meaning" i.e. referents of the strings of the calculus,) that meaning is not something that comes with the model itself: models are simply sets of structured entities and, as such, meaningless. What this suggests is that they have to have their meaning imposed upon them. But then, do they exist independently, ie. really, of the calculus of which they are the model?: "Models are not lost noumenal waifs looking for someone to name them; they are constructions within our theory itself, and they have names from birth." (H. Putnam, 1980, pg. 482). In this respect our conceptual spectacles tend to structure not only how, and what, we see in the world, but they also invent it.

[83] For an account, see Hogben, (1989, pgs. 243 ff.).

[84] This observation is made by Hutton, (C. Hutton, 1993, pg. 174).

[85] Z. Pylyshyn, (1991, pg. 234).

[86] An example in the natural sciences is the theory of relativity in comparison with the Newtonian account in respect of the problem of two particles moving under the influence of an arbitrary central force. "The elegance and generality of the classical solution ... is a delight. Relativity, though undoubtedly the more accurate theory, has nothing comparable to offer." (M. McCausland, 1997).

Nature's refusal to conform to canons of simplicity is particularly prevalent in biology: Wolpert remarks, in review of Brenner (1998), that "often in the history of molecular biology neither the simplest nor the most elegant theory turned out to be right."

[87] Recall that if there are no truth-issues involved in the choice between two extensionally equivalent grammars, then for those grammars, *their differences*, their distinct internal economies of information, or if you like, their intensional aspects, have only an instrumental status; for example, there is no choice, except in terms of some criterion of economy or elegance, to be made between any two grammars which achieve equivalent coverage of some fragment, e.g. let's say between a GB and a HPSG account.

[88] J. A. Fodor, (1985, pg 159).

[89] N. Chomsky, (1986, pg. 33).

[90] N. Chomsky, (1972, pg. 169).

CHAPTER IV.

ONTOLOGIES FOR THE LINGUISTIC II:
PSYCHOLOGICAL REALISM.

1.0 Introduction.

An explicit mentalism constitutes the third strand of Chomsky's revolution (the first and second, it will be recalled, being the mathematization of linguistics and the taking on of a scientific realism). In coming to focus on this mentalist strand, a strand we are to argue is unachieved, it is useful to keep in mind the basis of Chomsky's psychological claim, i.e. how psychological reality gets to be claimed for a linguist's grammar. The answer, in short, at least Chomsky's answer, is to assert that not only does a grammar characterize, on certain axes, the products of linguistic behaviour, but that a grammar is itself mentally represented, and it is in virtue of having a mentally represented grammar (an "MRG"), (as well as the relevant processing mechanisms), that we possess a language ability. The MRG is putatively involved in the aetiology of linguistic behaviour. It is, for example, the resource which is applied to in making judgements of acceptability, and it is, in standard parsing accounts, the knowledge resource to which the parser has access in the processing of strings. Moreover, in order to account for the facts of acquisition, language variation (within parameters that define a similarity) and linguistic creativity, the only apparently available option is to invoke some innate property of the mind/brain: a universal grammar that, given primary linguistic data, will output a grammar of the speaker-hearer of a language. The grammar does not just characterize, providing merely a constitutive account, but it also gives a sort-of-causal/reductionist explanation by virtue of relating properties of the linguistic to the psychological substrate, and this by way of the central theoretical construct, the MRG. In brief, the grammar, the characterization of the laws of the domain, is identified as (part of) the literally causal and reductionist explanation. The mental representation hypothesis is what supplies the psychological realism - it is how the linguist's grammar gets to be predicated as corresponding to an internal structure of the mind/brain.

These remarks rehearse the distinction we drew earlier between reductive and constitutive explanations. In the latter case, the terms and relations of the mathematical apparatus which articulates the explanation, as in Kepler's theory, are

not themselves required to be quantified over.[1] What are to be quantified over are all and only the linguistic objects, the set of sentences, picked out by a grammar. As long as the right set of such objects is picked out, then the apparatus that achieves this is in the same position as Kepler's laws of planetary motion: the particular economy of information of the notation is not significant; there may be neater formulations, but no truth issues hang on the choice of formulation as long as those formulations get it right, as it were, extensionally. In the case of an explanation that is reductionist/causal, where a MRG is postulated, then the economy of information of the calculus is significant, at least at some specified level of discrimination; it is something that is supposed to be in correspondence with "some determinate complex of neural mechanisms".[2r] This complex constitutes the facts of the matter beyond the problem of getting an extensionally accurate grammar, i.e. a grammar that simply achieves an efficient characterization: it is a matter of getting it right, where getting it right means formulating a grammar that corresponds to the mentally realized I-language, and so reflecting the economy of information of that I-language.[3] Interpreting the theory in this way, (there are, as we shall see, other readings), the data and their interpretation, i.e. the methodology and criteria adduced, need to be up to the task of fixing the content of the I-language to allow for a decision between competing claims for that content. Chomsky's contention in respect of E-language (Platonist/autonomist) approaches is that this 'getting it right' (i.e. going beyond a characterization of the set of sentences of a language) only makes sense in terms of a psychological instantiation of the linguistic: an explanation that addresses the question "why" as well as "what" is only to be achieved through a linguistics conducted from the perspective of "individual psychology."

The question we are to pursue is that of whether a competence grammar can be so upgraded from a characterizing vehicle (in answer to the question "what?") to being the central causal explanatory construct (in answer to the question "why?"); a mark of such success being whether there is any discernible difference in substantiated content between a linguistics conducted from an E-language perspective and one conducted from an I-language perspective. To anticipate our conclusion, and to indicate the direction we are to go in the following, our answer is that there is not. To put the point succinctly: the "abstraction" and "idealization" that Chomsky claims a requirement for enquiry effectively blurs the putative determinacy of the putative facts of the matter, the upshot being an account that is indistinguishable in substantiated content from an autonomist's/Platonist's constitutive and non-explanatory account.

1.1 Lines of enquiry: data and criteria of identity for theoretical constructs.

The distinction between causal/reductionist and constitutive types of explanation parallels the two ontological interpretations of the linguistic that we have canvassed: mentalism and autonomism/Platonism. One symptom of the difference between these positions is the opposing views taken on the question of the commensurability of the linguistic with the psychological. The ontological controversy correlates with the question of the nature of the scientific enquiry and type of explanation that is proper to linguistics. This difference is reflected not merely in respect of the ontological status of the object of enquiry, but also in the range of predictions that follow from the vehicle, the formal calculus, which expresses the theoretical claims; and conversely, in the range of data that are pertinent to the fixing of the content of a mentally realized competence grammar: for a linguistics undertaken from a psychological perspective there is no room for a restriction on the data to only speaker/hearer's intuitions. An upshot of the Chomskyan position is that there is a prediction of the relevance of data other than intuitions. The promise here is that such data may relate the linguistic to other domains: there is a prediction of the commensurability of the linguistic with the psychological.

The extent to which this prediction is borne out will be a mark of the extent to which a psychological realist construal of a linguistic theory is warranted and also the extent to which an I-language approach is contentfully distinguishable from an E-language approach. One of the marks of this availability of data will be that it will allow for decision between competing claims for the content of the MRG. Alternatively, one might require that other and appropriate criteria are adduced for decision between such claims, which criteria are appropriate in the sense of providing warrant for a psychological realism as regards the theory and its constructs.

The above remarks indicate one line of enquiry to be followed regarding the content, the economy of information, of proposed grammars. In brief, they take up the question of the realism of linguistic objects at a level more fine-grained than that of the set of sentences generated by a grammar, it is the question of how a decision is to be made between extensional equivalents. That there are appropriate means is a requirement for a psychological linguistics that posits a MRG, which MRG is a determinate structure and about which there are, putatively, determinate facts of the matter in respect of its economy of information. If there are determinate facts of the matter, then this latter claim can only be shown to be substantiated if these facts are, in principle, determinable. Ability to provide warranted content for a MRG is a

condition on substantiating the MRG hypothesis. The point is Quinean: if there is a determinate entity, then there are, or should be available, criteria of identity for that entity.

A second line of enquiry, not entirely separable from the former, and an issue which constitutes an unusual bone of consensus amongst philosophers of mind is the one raised by taking a grammar, a characterization of linguistic phenomena, as a causal explanation:[4] not only do acceptable strings of a language conform to the grammar, but the reason why they conform to the grammar is because the grammar is mentally represented and that MRG "provides the basis for actual use of language by a speaker-hearer."[5r] The essence of the philosophers' misgivings is that it is one thing to devise a grammar that accurately characterizes the sentences of a language, it is another to show that that grammar is causally related to the various forms of linguistic behaviour:

> The claim that the agent is acting on rules involves more than simply the claim that the rules describe his behavior and predict future behavior. Additional evidence is required to show that they are rules the agent is actually following, and not mere hypotheses or generalizations that correctly describe his behavior; there must be some independent reason for supposing that the rules are functioning causally.[6r]

To neglect such independent reasons would be to illicitly offer a constitutive account as a causal/reductionist account.

An egregious and revealing example of how this distinction is conflated is provided by Berwick's remarks regarding the nature of the explanation provided by a level one/competence theory of the visual system (Ullman's "rigidity assumption"[7r]):

> Suppose we take [the rigidity assumption] as the "best explanation" we have of how people compute the structure of an object viewing successive snapshots of it in motion. Then we can ask whether the Rigidity Principle is "used". Evidently, the answer is yes, even though we have no idea what its realization might be. Note that no one supposes that the Rigidity Principle is literally inscribed in the brain. Presumably, there is some physical basis for the Rigidity Principle itself, where the Rigidity Principle is not literally expressed. But this does not detract from the Rigidity Principle

> idealization, which successfully explains the operation of the visual system. The visual system apparently acts as if it used the Rigidity Principle.[8r]

The confusion here is over whether the principle is an account of "*how* people compute ... structure" or whether it is an account of *what* is computed. In sliding over and conflating the substantive distinction, the upshot is a case of taking an account of what is computed (which it properly is) and trying to smuggle it through as a causal account of how it is computed. A first bone of contention is that there is nothing at all "evident" about the Rigidity Principle being used (if we are taking "used" in some reasonably strict sense). Simply because behaviour conforms to the principle does not confer causal properties on it. (If the claim is that it is used, then is this not a claim that the principle is involved in the aetiology of the behaviour, and hence corresponds to some causal, mind/brain internal facts?) It is only "evident" if we are taking "used" in some unreasonably loose sense, a sense in which we might be willing to say, for example, that projectiles "use" the laws of classical mechanics. The second problem is that there is a world of difference between something being "evidently used", with a concomitant commitment to corresponding causal mind/brain internal structures, and it being the case that it is "as if it [is] used"; things that are claimed to be only "as if" are no more than fictions and fictions do not correspond to any real and causal entities. So we are left posing the question of what is the manner of the relation between the individual subject and the level one competence theory; just what type of explanation does a competence theory provide? Is it fictionally causal or really causal, and, if the latter, can we distinguish this claim from the former? Is it the case that it offers a characterization of behaviour in the way a Keplerian account of planetary motion sets out the laws which the planets obey, or does it also, at least in part, account for why the behaviour conforms to the "laws", and do this by a commitment to the internal representation of those "laws"?

It will be recalled that Kepler's account of planetary motion makes use of a mathematical vehicle, some set of formulae, which correctly characterizes that motion, stating its laws. The Newtonian explanation for that motion, a statement of why those laws are as they are, is provided by a further account of the nature of the stuff of which the planets are made. One may, as a *façon de parler*, take an intentional stance and say, as one does, that the planets obey Keplers' laws. However, these laws, *qua* some mathematical formalism, do not exist in nature, they are, if you like, mind stuff about nature; in nature you will find only the behaviour that conforms to those laws, you will not find, perhaps as some sort of inscription, the laws themselves.[9]

(Notice how it is precisely as some, despite Berwick's remarks, not very metaphorical inscription, as mentally represented grammars, that linguistic laws get, on a common interpretation of the Chomskyan scheme, to be themselves real, that is as corresponding to determinate neuro-physical structures with intentional content. The linguist's calculus, construed as an initially content-neutral, formal and instrumental means to model the phenomena and to articulate linguistic laws, has the status of *representans*, a representational construct, an *abstractum*. By way of the mental representation hypothesis it is reconstrued as *representatum*, a theoretical magnitude, an *illatum*. What we get is a literal expression of the Galilean assumption of the world as calculus writ large, and a correlative conflation of the conceptual apparatus that institutes discourse with the object to be figured by that discourse. The calculus is no longer in a modelling relation, as in the Keplerian case, rather a calculus is itself the object of study.)

Only fancifully, and by collapsing the distinction between the statement of the laws and the explanation of why those laws are as they are, might one say that the laws of gravity are referred to by massy bodies. These laws do not, however, provide the causal basis for actual movement by planetary bodies as a kind of knowledge resource or in the way a program is causally implicated in the behaviour of a computer. In the case of linguistics, what is needed are reasons and evidence to show that "attributing knowledge of a grammar is more plausible than attributing knowledge of the laws of physics to a projectile whose behaviour they predict."[10r] It is, of course, more plausible for the reason that language users are, standardly, the sorts of things that have minds, and minds are the sorts of things that possess knowledge. (The sceptic might equally say that it is more plausible by virtue of the fact that our ignorance about minds is such that there is very little that can be ruled out as obviously implausible.)

Plausibility, however, is one thing, good evidence is another and it is such that is needed to ground the claim that there are MRGs corresponding to real structures of the mind/brain. Furthermore, the plausibility becomes less marked when we consider the operations of a well-understood artefact such as an electronic calculator. It performs mathematical functions analogous to the linguistic function of mapping from an input signal to a structured linguistic representation. However, it is not the case that the calculator "knows" any mathematics in any usefully precise sense. There is no representation of mathematical knowledge, the mathematical functions it performs are what it *does*, not of what it has an internal representation.[11] In short, it is not an obviousness that to explain a cognitive ability one needs to posit an internal

representation, it is, however, how one gets to quantify over, i.e. to be mind/brain-internally psychologically realist about, the terms and relations of one's calculus. Of course, one may make such a hypothesis, but it is something for which proper evidence is required. We will argue that the strongest evidence that is adduced is that the mental representation hypothesis provides an apparently best explanation (in Harman's sense[12r]), but a best explanation that warrants no more than an "as if"[13] and so instrumentalist construal.

Chomsky's explanatory strategy is in keeping with the cognitivist paradigm in the brain sciences in general (or, perhaps, this is best the other way round: the Chomskyan manoeuvre is, historically, the model for cognitivism[14r]):

> A typical strategy in cognitive science has been to try to discover complex patterns such as those found in perception or language and then to postulate combinations of mental representations that will explain the pattern in the appropriate way. ... Epistemically, the existence of the patterns is taken as evidence for the existence of the representations. Causally, the existence of the representations is supposed to explain the existence of the patterns.[15r]

The positing of mental representations is the means by which to go beyond positivism (and Platonism/autonomism for that matter[16]) and so beyond a positivist linguistics, a linguistics that Chomsky has remarked would be no more than a science of meter readings:

> We as post-behaviourists know that explanations of behavior must advert to internal processes and we know of no other way of characterizing those processes except in terms of mentally represented rules.[17r]

Much the same is expressed by Fodor: "Take the notion of mental representation away from linguistic meta-theory and you get positivism by subtraction."[18r] In brief, the psychological claim is made on the back of a notion of mental representation and if this notion can be shown to be either lacking in cogency, or individual claims to be lacking in identifiable content, then the psychological status of the linguistic is unachieved. However, it is at least questionable whether the *sine qua non* for a psychological linguistics is this notion of mental representation, as if it were the only possible way the linguistic could be predicated of the mind/brain. (The only way we

can presently think of need not be the bound of possibilities, perhaps we need to be cleverer.)

The philosophers have provided arguments to the effect that indeed the notion of mental representation lacks the requisite cogency. Two arguments are those of Quine and of Searle.[19r]

2.0 Sceptical Representations.

2.1 Quine and mental representation.

Quine's argument focuses on the problematic scientific credentials of entities that are to be individuated in terms of their putative intensional properties, i.e. the set of rules (or parameters and constraints) that have propositional content, which content equates with one's putative knowledge of language. The problem is induced by the invocation of intensional properties such that the task for the linguist is to discover the correct set of rules, the mark of the correctness being that they correspond to the intensional properties of postulated mentally represented rules. This requirement, Quine argues, constitutes "an added burden" on the "grammarian" and, moreover, an added burden that cannot be carried through.[20r] The argument is, in essence, a re-working for syntax of his argument for the radical indeterminacy of translation.[21r]

Quine's point is that the hypothesis of a MRG has content if and only if it is possible to distinguish between competing proposals for the contents of the postulated MRG. This appears to require that these theoretical constructs are to be individuated in respect of their intensional properties. According to Quine such individuation is simply not possible (for the same reasons and in like manner that we cannot tell what the native "has in mind" when he says "gavagai"; maybe he means "rabbit" maybe he means "temporal stage of rabbit" or "undetached rabbit part" or any some such). Consequently, precisely because of the inability to choose between hypotheses in respect of their intensions, their economy of information, MRGs are not allowable scientific constructs, they are "*entia non grata*". On the grounds of "no entity without identity" we are encouraged to conclude that a MRG is no entity at all. And so, it would seem, with the disappearance of MRGs also disappears the possibility of a mentalist linguistics, at least to the extent that a mentalist linguistics cannot do without (cannot, to coin a phrase, get real without) predicating the mental representation of its

vehicle for articulating theory, i.e. by way of a notion of mentally represented rules individuated in respect of their intensional properties.

This argument is not, however, as has been pointed out severally,[22r] conclusive when it is taken as supplying *a priori* grounds for the inadmissibility of MRGs. In effect it is trying to stipulate in advance of science what will and will not be a viable scientific entity, it tries to exclude mentally represented grammars from inclusion in any future science rather like the apocryphal argument based on the best and soundest of first principles that showed, conclusively, that there can be seven and only seven planets in the solar system a little while before the discovery of the eighth.[23r] Indeed, in a more recent work Quine offers an implicit retraction of the strength of his original position: instead of entities, which are individuated by their intensional properties, being, on *a priori* grounds, indeterminate, rather the indeterminacy generating strictures are simply "traits of the science of our day [which] might well change as science advances".[24]

The central point that is rehearsed in the literature *contra* Quine's argument is that what that argument refers to is no more than common-or-garden underdeterminacy of a theory by the data. Nor is it the case that positing a MRG makes this underdeterminacy necessarily any more virulent than is standard in the natural sciences:

> For there can certainly be empirical evidence in favour of attributing to a speaker one underlying causal structure rather than another; ... [this because] as Chomsky often stresses ... there is no *a priori* limit to the kinds of evidence that might be relevant.[25r]

However, Quine's retraction and these observations do not vitiate Quine's point entirely. It is here that doubts about the cogency of the mental representation hypothesis merge with the issues raised above in respect of adducing the content of grammars such that there are appropriate means available to allow for a decision between competing grammars. We might also notice that we are also rehearsing Carr's reasons for disallowing a psychological ontological status for the linguistic and our own remarks about the lack of theory-external "observability" of theoretical constructs.

In principle, we are assured, there are all manner of types of evidence that might be relevant (standardly the list includes evidence from neurophysiology, acquisition, pathology and psycholinguistics), the problem is that, in practice, none of

these evidential types appear to get used in disconfirmation of grammars (we will be concerned with further substantiating this claim below). But if this is the case, and the MRG hypothesis is stranded from disconfirmation by these "relevant" data, then the evidential base is indistinguishable from that of Platonism/autonomism: the psychologism of the Chomskyan enterprise would be little more than an empty addendum leaving the relation of the linguistic to the psychological fairly well "a dangling, inexplicable, metaphysical fact" in much the same way as Carr's negotiation of the "fact" of interaction between the individual and the linguistic. This goes through unless an assumption is made that there *must* be a mentally represented competence grammar that is the psychological, mind/brain-internal reason why the natural linguistic is as it is.

While there is a sense in which there is indeed likely to be some predictively accurate grammar that achieves coverage (surely everything has an accurate description), the achievement of this is distinct from showing that any such grammar or some extensional equivalent has, in fact, a causal and explanatory role. In other words, one cannot conflate achieving coverage with achieving a psychological explanation of the linguistic, one still has to show the causal role of the competence grammar, which is, as we noted, just what the philosophers point out. If it is the case that the fixing of the content of the postulated MRG is stranded from the types of evidence alluded to (which evidence would be a mark of the commensurability of the linguistic with putatively related domains and potential evidence of the causal role of the MRG), then it is necessary that whatever other criteria are adduced are sufficient and proper to the task; we will need "clarification of [these] criteria."[26r]

This is where we return to Quine's argument but divest it of its *a priorism*: while there is nothing scientifically illicit about hypothesizing a MRG, if it is the case that there are insufficient means for fixing the content of a MRG (for only what is real is determinate and only what is real can have a causal role), then there appear to be several options. One, Chomsky's, is to preserve the hypothesis, license the abstraction of the discourse as being above any instantiating mechanisms and at the same time invoke some promissory brain-science to paper over the gap. Alternatively, one might go the direction, which is Carr's, of suggesting that there is no such available means because the nature of the linguistic is not to be explicated in psychological terms: the linguistic is not a psychological and mind-internal reality.

2.2 Searle and mental representation.

Searle's objections (directed at cognitivism in general) focus on the putatively ineliminably unconscious nature of knowledge of a grammar by individuals, what he refers to as the "unconscious intentionality" of linguistic rules. There are certain similarities with Quine's argument: Quine considers intensionally individuated entities to be slippery theoretic constructs, Searle unconscious intentional states. However, while Quine argues from considerations of criteria of identity for meanings, Searle argues, primarily, from a conceptual analysis of (un)consciousness by way of the notion of intentionality. The conclusion reached is that:

> The notion of an unconscious mental state implies accessibility to consciousness. We have no notion of the unconscious except as that which is potentially conscious.[27r]

He argues that the mark of an intentional state is that it has a certain "aspectual shape" (by which he means, more or less, a certain intension: a state may be about some object but it is always about that object "from a certain point of view"). This aspectual shape is only determinate from the perspective of the conscious agent and not from any third-person perspective. Moreover, (and this is where he goes the same direction as Quine, and where he can be challenged on the same grounds as Quine, i.e. stipulating in advance what is possible to science), "no amount of neurophysiological facts under neurophysiological description constitute aspectual facts."[28r] However, "the ontology of unconscious mental states, at the time they are unconscious, consists entirely in the existence of purely neurophysiological phenomena."[29r] The upshot is that states which are putatively always inaccessible to consciousness, e.g. the rules of a MRG, never have any aspectual shape, there is always and only a neurophysiological description possible, hence no description nor explanation in terms of the linguist's rules where those rules, i.e. intentional states, are posited to be causal of behaviour. The only sense to be made of unconscious intentional states is where those states are possible conscious states. If we want a cause of certain behaviour, then the strategy of invoking intensionally individuated and tacit mental representations is not a cogent option.

Notice that Searle's position does not take us back to a Platonism/autonomism for the linguistic, rather, on the one hand, it takes us toward viewing "explanations" in terms of MRGs as fictions and so, to the extent that such explanations are truth-

bearing by virtue of their making correct predictions, the constructs invoked require an instrumentalist, "as if" construal. On the other hand, it leads toward asking for a neurophysiological (and so an eliminative reductionist) explanation of the linguistic and one which excises all realist talk about mentally represented rules.[30]

One might, or might not, be impressed by Searle's argument as constituting an *a priori* dismissal of mental representations of tacit knowledge. A reason why one might not, even if one can find no obvious flaws, is, as in the Quinean case, that such argumentation depends, if it is to be irrefutable, on the unrevisability of the concepts whose analysis provides the premises that take one to the conclusion. As already remarked, one aspect of the scientific enterprise is that it "may reconfigure our current assumptions"[31r] and our current concepts. The point, indeed a Quinean point, is that:

> Philosophers don't get to tell you what counts as [a] permissible scientific construct, ... What determines which constructs are permissible ... is: how the world turns out to be. We will find out whether we can make sense of 'mental representation' *as we go along*.[32]

Taking this on board, the line to be taken will be to examine the extent to which the mental representation hypothesis, by achieving corroborated content, achieves an explanation and so makes sense of the concept. We might, however, also take Searle's argument (as well as Quine's) as making a prediction that this will be unachieved. It will be unachieved, i.e. we will be unwarranted in construing the grammar as mind/brain-internally real, if the content of the postulated MRG cannot be fixed, or if we cannot adduce evidence for the causal role of a MRG in linguistic behaviour. On both counts such failure would bring to question the reality of a MRG. (Notice that this is distinct from bringing to question the notion of mental representation in general: the point is not so much that there is no mental representation as that some particular system is not mentally represented.)

3.0 To what is a grammar to correspond?

Central to Chomsky's claim that a linguist's grammar is psychologically real is the assertion that a grammar is "correct insofar as it corresponds to the internally represented grammar."[33r] In this way the task is to explicate the nature of the mind-internally represented linguistic knowledge, the I-language, which constitutes what one

knows when one knows a language. A problem here, and what amounts to bringing to question the mind-internality of the psychological claim, is that there is a studied, and purportedly principled, vagueness about exactly what is required by getting one's grammar to "correspond":

> Exactly what is meant by the notion "corresponds" in the case of the abstract study of a physical system is a complex question, not unique to this enterprise.
>
> The statements of a grammar are ... about structures of the brain formulated at a certain level of abstraction from mechanisms.[34r]

("Mechanisms" here referring, presumably, to the neural structures that instantiate the linguistic information.) This vagueness, brought in by way of the "abstraction" of the enterprise, (and which, for George, gives rise to "confusion"[35r]) has led to claims that Chomsky's position is "ontologically indeterminate", on the grounds that this abstraction constitutes a contact breaking move from any contentful notion of the psychological, where this is construed as concerned with mind-internal states.[36r] What the invocation of "abstraction" arguably and suspiciously achieves is the ability to maintain the hypothesis of a grammar as mind-internal and causal while, at the same time, the "abstraction" of the description distances the thesis from any evidence (and justifies the lack of evidence) as to the mind-internal, causal role of the grammar.

There are two related ways in which one can figure this issue. Firstly, in terms of an ambiguity in the notion of "knowledge". In asking that a linguistic theory correspond to knowledge of language are we asking for an account of what is known by a speaker of a language, (i.e. the object of knowledge considered independently of any knowing subject,) or to the knowing, by the individual, of that knowledge? Knowledge is one of the propositional attitudes. To know x is to be in a psychological state that relates an individual to x. One might be tempted to take it that this is sufficient to ground the psychological (mind-internal) ontological status of x, where, for instance, some linguistic knowledge is the value of this variable. However, while the knowing is psychological, the object of knowledge need not have a psychological locus, as is standardly the interpretation of mathematical objects, or, indeed, of real world objects in general. The incompatibility of properties of the object of knowledge (e.g. its infinitude in both the case of mathematics and linguistics) with its being accorded a psychological ontology is one of the weapons in Katz' armory as to why

the two sides of the ambiguity cannot be conflated, and as to why the linguistic *qua* object of knowledge, the proper concern of the linguist, is distinct of the psychogrammar which is the contingent, psychological basis for the relation between an individual and that object.[37]

Alternatively, one might understand the vagueness in terms of a distinction between the rules of a grammar being implicitly or explicitly represented; in this case the vagueness infects the notion of representation:

> [Chomsky's] writings have not acknowledged the distinction between the rules in a system being *explicitly encoded*, and a system merely implicitly *conforming to* rules - i.e. behaving as if it were following rules even though the behavior may arise from unspecified causes.[38r]

The same point, is made by Stabler in terms of the rules being "encoded" in the manner in which a computer program directs the running of a computer and, on the other hand, being "hard-wired", somewhat in the manner that a calculator performs mathematical functions without any explicit representation of the mathematical "rules".[39r]

The issue brings to light a certain tension in Chomsky's position, a tension that parallels the move from linguistics construed as a characterizing enterprise to linguistics taken as providing a psychologically (mind-internally) real account; it is a question regarding the reality of the constructs of the theory, of the correspondence of the terms and relations of a calculus to some real and mind/brain-internal entities.

It is possible, in Chomsky's writings, to find passages, some of which appear to support one reading and some the alternative. For example, that Chomsky is positing the explicit representation of rules is the most accessible interpretation of the following assertions:

> The grammar is mentally represented and used [like a computer program is used?] in the exercise of linguistic abilities such as understanding speech and making grammaticality judgements.[40r]

> [A grammar is a device that] assigns to each expression a structure, which we may take to be a set of representations, one on each linguistic level, where a

> linguistic level is a particular system of mental representation.[41r]

On the other hand (my italics):

> A generative grammar attempts to characterize *in the most neutral possible terms* the knowledge of the language that provides the basis for actual use of language by a speaker-hearer.[42r]

> The grammar, *in whatever form its principles are represented in the mind or brain*, simply characterizes the properties of sentences, much as the principles of arithmetic determine the properties of numbers.[43r]

The tension here marks a substantive issue concerning the nature of the explanation attempted and, correlatively, the existential commitment of a theory.

When representation is taken in an implicit sense, the economy of information of a grammar has the same instrumental status as predictively equivalent but notationally and "intensionally" distinct formulations of the laws of mechanics: as long as they make the same correct predictions there is no further truth-issue at stake. Similarly, in the case of a calculator, there may be different ways to express the functions computed, but there are no truth-issues involved in respect of these different formulations (compare with our remarks on choosing between grammars from the Platonist/autonomist perspective). Nor, however, in the case of the calculator, does the stating of the function supply any more than a characterization of the output of the machine: the characterization, the specification of the function computed, cannot, by itself, be taken as an explanation of the output: the cause remains unspecified.

Certainly, one might propose an explicit representational hypothesis, i.e. that the functions computed by a calculator are internally represented as a body of propositional/declarative knowledge, but, as it turns out, while this would be predictively successful, it would also be the wrong explanation. At least, it is the wrong explanation unless we are using "representation" in a sense so loose as to be more or less vacuous, and vacuous because it could not engender any useful predictions as to the internal workings of the calculator. It is only in a somewhat "as if", metaphorical ("abstract"/"neutral") sense that a calculator represents mathematical knowledge (in the same way that it is only "as if" a projectile consults the relevant laws when it knows how to describe a parabola). If this is the sense intended by

Chomsky, then Searle's objections to the representational hypothesis are misplaced, because an "as if" view of the intentionality of the system of rules does not constitute a commitment to represented tacit/unconscious knowledge.

However, this implicit sense of representation is inconsistent with other aspects of Chomsky's position. Chomsky's claim that:

> Substantial coverage of data is not a particularly significant result; it can be attained in many ways and the result is not very informative as to the correctness of the principles employed.[44r]

only make sense if there are facts of the matter at stake (i.e. certain truth-issues regarding what is mentally represented,) in respect of the choice between extensionally equivalent grammars. The same remark applies vis-á-vis the position that Chomsky takes in the debate *contra* Quine's doubts over the scientific credentials of theoretic constructs that are to be individuated in terms of their intensional properties. Furthermore, the implicit construal of representation appears inconsistent with Chomsky's emphasis on providing criteria of adequacy in order to allow for decision between extensionally equivalent grammars; again this only makes sense if the claim is that there are determinate, representational facts of the matter that it is the task of a linguistic theory to explicate.

Somewhat bizarrely, ("bizarrely" because he takes it as "a positive note",) Stabler remarks that the implicit/explicit issue is rather academic and by the by:

> Even if the representational hypothesis were removed from current theories, very little of the substantial and interesting work that has been done in linguistics and psychology is going to topple as a result.[45r]

On a negative note, exactly the same coin, just the other side, this suggests that the representational hypothesis is, for both linguistics and psychology, empirically vacuous; something of an idle cog that gets you to claim linguistics to be one of the natural sciences, but which otherwise does not have any identifiable content. Furthermore, for the same reasons, the upshot would be that there would be no evident, contentful distinction between a psychologistic and a Platonist/autonomous linguistics, because it is the representational hypothesis that gets you the psychological (mind-internal) content: recall Fodor's observation that to "take the notion of mental representation away from linguistic meta-theory [is to] get positivism by

subtraction".[46] In avoidance of this, the question for Chomsky is one of how much mileage one can get out of, and refuge one can take in, the notion of "abstraction from mechanisms" such as to allow the maintenance of the causal, explanatory and psychological claim without being able to provide any content for it.

To provisionally sum up: Chomsky's psychological realism, to be made good, requires that the linguist's grammar corresponds to some psychological, mind-internal state of affairs; to some psychological structure that represents, at some level of discrimination, the same informational content as that grammar. In short, the truth of the grammar-as-sort-of-causal-explanation hypothesis (as opposed to the grammar taken as a constitutive, characterizing account) depends on the reality of mental representations. For this to be testable, (we are, of course, as scientists, committed not only to the attempt but to the possibility of disconfirmation of our theories; this is why this is science and not pseudo-science) we need, firstly, some specification, sometime, of our ontological commitments, in other words, a specification of how the terms and relations of a grammar map onto the internalized representation (i.e. what is theoretical magnitude, what representational construct), and, secondly, that competing claims can be decided between in respect of available data and/or appropriate criteria, and without engaging in the theory-internal hermeneutic circle which we described. If there really is a MRG, then there will be a set of determinate and determinable facts of the matter. That is why there are supposed to be truth-issues involved in respect of extensionally equivalent grammars. If, however, there are no such things as MRGs such as constitute the facts of the matter, then also disappear the supposed and correlative truth-issues. Nor, of course, does what is not the case provide data to allow for decision between competing theories. Decision between competing extensionally equivalent grammars (that are truth-bearing by way of making correct predictions over the external behaviours) would require, as we will see it is reduced to, resort to the type of theory-internal, methodological and non-truth-relevant considerations that are appropriate to the choice between different notations.

At first blush, it would seem there should be no need for such resort: given that MRGs are putatively psychological, the promise is of the realization of the repeatedly canvassed relevance of the brain sciences to the disconfirmation of linguistic theories: "facts about the brain [could] select among theories ... that might be empirically indistinguishable in other terms."[47r] The relevance of such facts being a mark of the commensurability of the linguistic with the psychological and a means to achieve a certain theory-externality. Furthermore, one might be tempted to the view,

given the putatively causal role of the grammar, that it more or less directly feeds into the operations of the performance mechanisms, i.e. the information of the grammar is more or less isomorphic with that of the parser. If such were the case, then psycholinguistic experiments would be appropriate to the task of (dis)-confirming the relation of correspondence between linguists' grammars and the MRG.

3.1 Choosing between grammars I: the (ir-)relevance of psycholinguistics.

The suggestion of the relevance of psycholinguistic data to the confirmation of grammars was made (first, to my knowledge,) by Miller and Chomsky:

> The psychological plausibility of a transformational model of the language user would be strengthened, of course, if it could be shown that our performance on tasks requiring an appreciation of the structure of transformed sentences is some function of the nature, number, and complexity of the grammatical operations involved.[48r]

The result of this suggestion was the Derivational Theory of Complexity (DTC). In brief, the idea was that processing time should correlate with the number of transformations involved in the derivation of a sentence; passive interrogatives, for instance, being predicted to require greater processing time than the corresponding active affirmatives. Unfortunately, despite some initially promising findings, the theory got buried.[49r]

What was also buried was the direct relevance of psycholinguistic research in respect of the confirmation of linguists' grammars. The conclusion reached was that:

> The human sentence processing mechanism does not make direct use of the rules of the mental grammar studied by linguists .... [Consequently,] we can learn nothing about the format of the mental grammar by studying language processing.[50r]

Rather than the evidence being taken as disconfirmatory of a transformational grammar, instead the evidence was taken as disconfirmatory of the null hypothesis (i.e. of the isomorphic relation between grammar and parser). It also gave rise to an explicit disclaimer from Chomsky:

> There is not the slightest justification for any such assumption [of an isomorphism between grammar and parser, and hence of the grammar as a performance model]. Such an assumption seems not only without justification but entirely counter to whatever vague notions one may have about the processes that underlie production.[51r]

This makes a lot of psycholinguists very unhappy (for it makes them rather peripheral to the main action) and some quite openly disbelieving: the complaint is that if, as claimed, "a reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of the language",[52r] then the competence grammar is used in performance. And, if it is used, then the study of performance cannot but be directly relevant to the task of adducing evidence. Consequently, on this reasoning, (which is Bresnan's and Kaplan's:) "to the extent that there is evidence against the DTC, this can be taken as evidence against the psychological reality of transformational grammars."[53r] To assert the contrary is, according to Steinberg, to be guilty of "a psychological self-contradiction."[54r] In brief, the mark of the psychological reality of a grammar is just its observability in performance, its being accessible to psycholinguistic disconfirmation. This is a view that persists and gets rehearsed, to greater or lesser degrees, in the literature (e.g. Kintsch, (1974), Bresnan, Halle and Miller, (1978), Steinberg, (1993), and, a philosopher, Soames (1985)). Alternatively, to preserve the claimed relevance of psycholinguistic research to grammar disconfirmation, it is necessary to insist that it is "plausible to reinstitute the methodological assumption that the mental grammar directly guides the parser's behavior."[55r]

In the former case, (Steinberg's (*et al.*'s) militant psycholinguists) the claim boils down to the assertion that if a linguistic hypothesis is not amenable to psycholinguistic research, then the hypothesis is not psychological. (And, of course, all is as it should be if the linguistic is autonomous of the mind, if linguistic objects are, for example, platonic objects.) This has, I suppose, a degree of *prima facie* reasonableness: a putatively psychological linguistics that is irrelevant to psycholinguistics appears, at best, anomalous; for surely, if any brain sciences are to be relevant, psycholinguistics is the prime candidate.

However, such considerations should not immediately convince one of the irreducibility of competence theories to psychology, rather, the interpretation is plausibly that these considerations merely defer the achievement of psycholinguistic

content. The reason being that this militancy need amount to little more than "the rage of Caliban at not seeing himself in the mirror." It is a case of attempting to claim proprietary rights over what counts as psychological, as if the psychological status of a theory were dependent on it being a theory that is testable by the (let's face it) rather hit and miss techniques of current psycholinguistic experimentation.[56] This militancy would only make sense if psycholinguistics were something approaching an achieved enquiry. Moreover, given that psycholinguistics, as presently conducted, is centrally concerned with processing, the implicit claim is that the psychological domain is defined as concerned exclusively with processes; a case of, in George's phrase, "process fetishism": if its not a process, its not psychological.[57] The upshot is a species of positivism where the psychological domain is defined as being restricted to those phenomena that are accessible to current techniques of verification and observation.[58]

The second problem with demands that psycholinguistics have a privileged role in the disconfirmation of competence theories is that it depends on the assumption of the null hypothesis, i.e. of their being some fairly direct relation (approaching isomorphism) between the competence grammar and the parser, that "the parser and the grammar are well-matched."[59r] Unfortunately, while there is:

> a measure of informal agreement about what would be a more direct, or a less direct, way of putting a given grammar to work ... [at the same time] there is no precisely defined notion of a "direct" processing implementation of a given grammar.[60r]

The difficulties that result from this imprecision are exacerbated by the fact that, even if there were a clear notion of direct use of a grammar by a parser, this would be of little use: Berwick points out[61r] that should a parser be in a logically direct relation with the grammar this need not translate into psycholinguistic evidence (given current techniques) being relevant to disconfirming the competence theory, because the temporal performance sequence need not correspond to the logical sequence of operations in a derivation.

The disconfirmatory status of psycholinguistics vis-à-vis competence theories becomes yet more tenuous when we consider that the null hypothesis is itself somewhat lacking in motivation (other than that its the only way to obtain the disconfirmatory status of current psycholinguistics), for "there is no inherent reason why the parsing process must correspond directly to the form of the grammar."[62] Indeed, there are several, clearly defined, possible non-isomorphic relations between

grammars and their parsers, none of which can be immediately excluded as psychologically implausible (see, for a catalogue, Abney (1988) and van de Koot (1990)).

The unavoidable and principled conclusion is that competence theories are distanced or deferred from disconfirmation by current psycholinguistics. However, despite these well-motivated grounds for putting some distance between linguistic theories and psycholinguistic confirmation, this is no particular reason to be sanguine about linguistic theories' psychologism. This is because the net result is that the discourse domain of competence theories is insulated from, and so unfalsifiable by, data from the very domains that are predicted to be and should be relevant. Reasoning, no matter how cogent, to the conclusion of the unfalsifiability of theoretical (and ontological) claims in respect of some evidential domain is at best a pyrrhic victory: not so much a solution but a problem. The problem is that, apparently, the only data that are, in practice, pertinent to the development, the fixing of content and the testing of competence theories are speakers' judgements of acceptability. But, as Chomsky notes:

> It seems absurd to restrict linguistics to the study of introspective judgements [for such would be to] reduce the field to problem solving .... That is, perhaps, the natural definition if you abandon any [psychological] realist conception of the field.[63r]

The question that arises, is how it is possible, in the face of this empirical strandedness from psychology, to maintain the psychological ontological status of the linguistic? One answer is to point to the comparative lack of development of the appropriate brain sciences, and so, by implication, to invoke some promissory neuroscience. An alternative is to invoke a psychology that is not defined in terms of an exclusive concern with mental processes, one which is justificatory of the level of abstraction above mechanism that is deemed appropriate.

While it is perfectly cogent to point to the differing levels of development of linguistics and the brain sciences as grounds for their present incommensurability, one may be less happy about the slant given; it is one which seems designed to maintain the aforementioned insulation of competence theories from disconfirmation. Competence theories, it appears, set the task for the neurosciences but not vice versa: "results obtained at this [competence] level provide a guide for the study of mechanisms":[64r]

> It is the task of psycholinguistics to develop processing theory along lines that will be comparable to linguistics. When such theories become available, then and only then can serious connections be made between psychology and linguistics - only at this level of abstraction will any significant convergences be discovered.[65r]

Note that the onus is not on linguistics to specify the relation between competence and the instantiating mechanisms ("mechanisms" being ambiguous between the representing "neural structures" and the performance device), rather the onus is on some future psycholinguistics or psychology to show how the competence theory *qua* MRG gets realized.

But what sense of "psychological" is this for which a promissory psychology is obliged, on the basis of some sort of necessity, to make good the mental representation hypothesis? One or the other (or, as it turns out, both) of two options can be taken: on the one hand, one can exchange a deferred and promissory psychology for a never-never psychology; on the other, one can invoke a sense of the psychological to parallel the canvassed abstractness of the correspondence between the linguist's grammar and the putatively mental object that it describes, one which is abstracted above causal and instantiating structures.

The advantage of the former strategy is that it allows one to deal with the discomfort of having to maintain a realism over the constructs of a theory that are unavailable to any disconfirmation by the supposedly relevant adjacent domains. It is a manner in which to negotiate unfalsifiability (by evidence from these domains) with impunity. The way to making this sound respectable is to invoke, as we discussed earlier, some notion of "epistemic boundedness" (or "cognitive closure"), i.e. there may be some phenomena that we are intellectually and cognitively unequipped to account for, some phenomena are simply beyond our capacities to understand and explicate. A prime candidate being the instantiation of a grammar by neuro-physiological structures. In effect, it is a way to explain away the anomaly of a psychological linguistics that is incommensurable with the rest of psychology and the brain sciences: the anomalousness, by virtue of being reasoned, is not really anomalousness after all. Taking this line gets Chomsky to cite, a little surprisingly, perhaps, Wittgenstein (that noted mentalist) with approval:

> It is ... perfectly possible that certain psychological phenomena cannot be investigated [neuro-]

> physiologically because nothing [neuro-] physiological corresponds to them.[66]

The problem with this line is that there is something more than a little disquieting about a theoretical position that depends in part, for the maintenance of a realist claim, on invoking this idea of epistemic boundedness where it is used to justify an inability to adduce potentially disconfirming data. It is also the case that much the same lack of correspondence would result if it were simply the case that there were no such psychological entity as a competence grammar (which is, I suspect, a little closer to what Wittgenstein's views on the matter would have been).

The second strategy is to justify the practical unavailability of data other than speaker judgements on the grounds of the abstraction of the level of discourse "above mechanisms" that is deemed appropriate. And it is appropriate because our present ignorance of the nature of the mind/brain precludes the theorist from usefully proposing any more concrete hypotheses regarding the relation between competence grammars and performance, on the one hand, and competence grammars and their manner of realization on the other. In other words, the relations between competence and instantiating neural structures, and between competence and performance are left as blanks (compare Fodor's observation of "enthymemic explanation" in cognitive science).

The misgiving over the taking of this line is that it is not immediately obvious how this abstraction over the putative causal, instantiating mechanisms is to be contentfully distinguished from no more (and, of course, no less) than an abstract characterization of the patterns evidenced in behaviour. The abstraction appears to constitute a contact breaking move from other, supposedly relevant, domains of discourse, making the theory's constructs both "ontologically indeterminate" and insulated from data in adjacent fields.[67] Indeed, is there not something altogether rather suspicious about a move that allows just such insulation. Even more suspiciously and to our point, such insulation is precisely the mark of a domain that is ineliminably autonomous and ontologically discrete. What we get, again, is the "dangling, inexplicable, metaphysical fact" of the relation between the linguistic and the psychological, exactly recapitulating Carr's autonomist account (or lack of account) of the "fact" of interaction between the individual and the linguistic. If we want some firm content to this fact, and content to the psychological ontological claim for the constructs of the linguist's theory, then this abstraction appears to constitute an institutionalized obstacle, blurring any firm criteria of identity for a theory's constructs.

In sum, given Chomsky's claim that there is a relation of correspondence between the linguist's competence theory and the postulated internally represented grammar, then there is a requirement on a theory to address the question of this relation. And this because we need to know not only if there is a competence grammar (i.e. an accurate characterization of linguistic knowledge), but whether a competence grammar, a body of propositional knowledge that is mentally represented is indeed properly in causal (as opposed to constitutive) explanation of the nature of natural languages. The way towards this is through some firming up of the abstraction of the discourse by specifying how, at least at the level of the economy of information, the competence grammar, the knowledge of language, is realized. In other words, what is to be addressed is the question of "the realization relation" between the grammar and an individual psychology.[68r] For if the theory, the claim, is being pitched at the level of individual psychology, then only:

> so long as the claim itself is well-defined and has truth conditions for its being true [at this level], [does] it remain a perfectly sound scientific proposition.[69r]

If there are determinate mind/brain internal facts of the matter concerning a MRG, and if a theory that postulates an MRG is to be testable as a thesis of the mind-internal, constructive basis of the nature of natural language, then such a theory's level of discourse has to be pitched at a level appropriate to what is being claimed. Peacocke's "Informational Criterion"[70r] is an attempt to elucidate what that level is such that a theory meeting the criterion would be contentfully a theory with a claim to psychological reality.

3.2 Peacocke, Marr, cognitivism and level 1.5.

Peacocke's position is interpretably a rehearsal, only rather more subtle and coined in cognitivist terms, of Bresnan's previously canvassed solution to the problem of choosing between extensionally equivalent grammars (i.e. the assumption of an isomorphic relation between grammar and parser). It might also be thought of as rather in the manner of a supplement, or alternative, to Chomsky's criteria of adequacy.[71r]

One way of presenting the problem that Peacocke addresses is in terms of the observation (the Quinean observation) that competence theories can be characterized

in too many ways, this is what constitutes the difficulty for substantiating the claim to there being any relation of correspondence between a linguist's grammar and a determinate MRG, it is what constitutes the difficulty of a psychological realist construal of the terms of a linguist's grammar: there are insufficient criteria of identity and without such we are disallowed from the postulation of a corresponding entity.

Before we consider Peacocke's proposal it will be useful to sketch out the relation between the Chomskyan and cognitivist paradigms.

The Chomskyan distinction between competence and performance, the former constituting a discrete and primary object of study, provides the model for Marr's characterization of the "Natural Computational Framework". Marr identifies three levels of description relevant to theories of cognitive function: the first level, level one, specifies the function computed (correlating to a competence theory), level two identifies the algorithm which performs the computation (a parser), and level three the hardware realization (the neurophysiological structures).[72] The presiding insight is that the mind may be understood in much the same way that we understand the functioning of a computer: cognition in general is seen as an instance of running a program that is instantiated in the mind/brain. Linguistic competence is then a matter of possessing the relevant program; what we have when we know a language is a computational system (quite explicitly so in the Minimalist program). The placing of linguistics within the more general cognitivist paradigm is endorsed by Chomsky for the two enterprises share:

> the belief that certain aspects of the mind/brain can be usefully construed on the model of computational systems of rules that form and modify representations, and that are put to use in interpretation and action.[73r]

Parallel to the case in Chomskyan linguistics, the consensus is that the general task of providing an account of cognitive function is best pursued by taking the issue of the level one function as prior and distinct from a specification of the processing mechanisms. Marr remarks that: "Finding algorithms by which Chomsky's theory may be implemented is a completely different endeavour from formulating the theory itself."[74r] The rationale for this competence-first strategy being that it is only when we have a clear idea of what the cognitive function is that we will be able to usefully attempt an account of how it is computed. It is in the manner of a heuristic:

> The level of abstraction [,the level of the function computed,] is appropriate insofar as the results obtained at this level provide a guide for the study of mechanisms, much as study of chemical properties provides a guide for inquiry into atomic theory.[75]

Significantly, the Marr hierarchy does not require that level one theories are mentally represented in any explicit sense, nor is the coherence of this hierarchy of description affected by this neutrality on the representational issue. However, what is affected by this neutrality is the nature of the explanation provided by a level one theory: the question is just what is a level one theory true of by virtue of its correspondence to (and predictive success in relation to,) the facts of the observed behaviour? Need a predictively accurate level one theory be any other than an abstract characterization over the observed behaviour for which there are no truth-issues involved in respect of extensionally equivalent formulations? If it need not, then it also need not be true in virtue of corresponding to a mentally represented cognitive resource that is "put to use in interpretation and action", but true in virtue of being simply a characterization of the observed patterns of external behaviour, the cause of which is, presumably, the (unknown) nature of the internal states of the mind/brain, but about which states nothing has been specified. This would amount to an explanation akin to a Keplerian account of planetary motion; it would constitute a statement of the laws that the behaviour obeys, but without furnishing any reasons as to why (nor how) the behaviour conforms to such a characterization. Such an account would be psychological in a mind-external/object-of-a-propositional-attitude sense, i.e. it is about the products of behaviour which, presumably, have some mind/brain internal cause, but, it does not invoke the sense of "psychological" where what is referred to are internal states of the mind/brain.

These are the issues that Peacocke addresses. His "Informational Criterion" reflects the contention that for a level one theory to be contentfully proposed as part of a mind/brain-internal, causal account, such that it can be distinguished from a characterization of the products of linguistic behaviour, it needs to firm up the truth-conditions as regards what would warrant the construal of a theory as causally involved in linguistic behaviour. These truth-conditions reflect the need to show a relation between a level one theory and mind/brain internal states. Correlatively, the issue is one of whether the question of the realization relation, the relation between the different levels of description, needs to be addressed before claims to

psychological realism (in the mind/brain-internal sense) for a level one description are appropriate, for, as van de Koot, observes:

> At least some relation between the three levels must obviously obtain if claims about the (psychological) reality of competence theories are to have any content.[76r]

Peacocke argues, in line with our remarks above, that a level one theory "obliterate[s] [is abstracted away from?] distinctions that correspond to psychologically real differences."[77r] There are psychological facts of the matter that are not captured at the level of the function computed and these facts are, Peacocke contends, crucial to the contentfulness of the claim to a theory's psychological reality. The particular facts that Peacocke focuses on are those regarding the realization relation for competence theories, in particular the relation between the competence theory and the performance mechanisms, in other words, facts concerning "the information drawn upon by the relevant mechanisms or algorithms in [the] subject."[78r] These facts, the ones to be addressed, are neither specified by the level one function computed, nor are they specifying of any particular algorithm (although they specify an "an equivalence class of algorithms, namely, all those drawing on the same body of information"[79r]). This level of description is dubbed level 1.5 in terms of Marr's scheme, and it is this level of description that is deemed appropriate: "the claim of psychological reality for a rule of grammar is a claim at level 1.5."[80r]

What this buys is "a form of causal explanation": the reason why linguistic behaviour exhibits the structures that it does is because the relevant mechanisms draw on this particular economy of information, it is this information that is used and the sense of "used" here is no "as if" sense. Peacocke's proposal is an attempt to set the conditions on excising the "as if" from the explanation. What is being specified is the manner of the relation between the object of knowledge and the subject who evidences linguistic behaviour. In effect, what Peacocke is addressing is the problem, observed by Fodor, of "Neo-Cartesian", enthymemic explanations in cognitivism where precisely this relation is left unspecified; one waves one's hands in the general direction of the performance mechanisms which "do for Chomsky some of what the pineal gland was supposed to do for Descartes".[81r]

What is not clear in Peacocke's discussion is whether the Informational Criterion is to be taken as the sufficient and necessary condition on the psychological reality of a grammar. Is this the only relevant criterion? Furthermore, although it

might be desirable, and in the long run a requirement, that a theory specifies its realization relation, is it necessary or, indeed, is it a mistake to try to provide such a criterion prior to empirical enquiry?

In respect of the former of these questions, if the criterion is canvassed as being the single necessary condition on the psychological reality of a theory, then, as George points out, this merely reflects the previously remarked on illicit identification of psychology with mental processes; a case of "appropriat[ing] the term 'psychology' for the study of a restricted subset of mental life," and assuming that "algorithmic processing [is] a touchstone for psychological existence."[82r] But then, taking George's point on board, although we can blithely and reasonably get to call the level one description of cognitive function "psychological", what we get to call a study is really not the point. The point is the issue of the constructive role of the mind/brain in relation to cognitive function, and it is just this issue that a level one description does not directly address, even though it is, arguably, a necessary preliminary. It is even more the point when we recall Chomsky's bet that it is mind/brain internal states which are hypothesized as constructive of the nature of natural language: once we have a complete understanding of the brain sciences then "there is not the slightest reason to suppose that there are truths of language that would still escape our grasp."[83r]

Even though George's observations are cogent they do not solve the problem, for the relation between the competence theory and the internal state of the mind/brain vis-à-vis processing mechanisms remains unspecified and without clear truth-conditions. As such the competence theory remains both explanatorily and ontologically dangling. The former because it is not shown to do any causal work, and the latter because without substantiating the relation between the competence theory and the performance mechanisms, for which it is standardly considered to be some sort of informational resource, it remains also unsubstantiated whether there is, in fact, any mind-internal structure that corresponds to a competence grammar. The competence grammar is incommensurable with and insulated from other discourse domains. This is, of course, the evidence that Carr adduces for his view that the linguistic is ontologically autonomous. In turning to the second of our questions (is it a mistake to try to provide criteria prior to empirical enquiry?) and the manner of its negotiation, this upshot, this empirical strandedness, does not get excised, rather it is defused.

Interpretably, Peacocke's criterion, in requiring of a competence theory that it specifies how the information, at level one, feeds into the processing mechanisms,

parallels Pylyshyn's requirement that: "the theory that accompanies the notation must tell us which aspects are intended to be empirically significant."[84r] To put this into the terms of our previous discussion, the requirement is to specify what elements of the notation are to be taken as corresponding to real and, in principle, theory-externally observable linguistic entities; what are the theoretical magnitudes, the posited *illata,* and what the representational constructs, the *abstracta.* Pylyshyn's complaint is that such a specification is not offered. In brief, Chomsky fails to address the question of the realization relation of a competence theory.

However, this is not simply negligence or an oversight, rather it is in recognition of the fact that any hypothesis as to the manner of the relation between an individual and a competence theory would be otiose given the present state of development of the brain sciences. It is this lack of development that justifies the "certain level of abstraction from mechanisms". We are encouraged to view linguistic enquiry as not dissimilar to that of nineteenth century chemists into the properties of the combinability of different elements where it would have been unreasonable to demand of them that they also supply the sub-atomic theory that explicates the relation between the chemical property of valence and its instantiation in a chemical element.[85r]

Peacocke disputes the analogy.[86r] (He misses a trick, however, in not pointing out that the theories that the nineteenth century chemists came up with did not *have* to be true, but only got to be confirmed, in mutually confirming retrospect, by sub-atomic theory. Chomsky's analogy is somewhat tendentious, change it and you get a rather different picture: why not consider the view of linguistic enquiry as not dissimilar to that of eighteenth century scientists into combustion and then think what happened to phlogiston? One does not get a warranted realism for the constructs of one's theoretical domain by fiat, but only by some suitable corroboration.) The Informational Criterion, Peacocke insists, is not a requirement on linguists that they supply a neurological theory, rather it is a requirement on linguists that they specify just what economy of information is drawn on by the processing mechanisms, or, in more or less Pylyshyn's terms, a requirement that linguists specify what in the notation is algorithmically significant. Even though we have been led to dispute the insistence on algorithmic processes as *the* mark of the psychological, they are *a* mark of the psychological, (i.e. members of the set of psychological phenomena: if the psychological constitutes a nomological domain, algorithmic cognitive processes fall within that domain), and the general point remains: the brain sciences, even if they are only promissory, need to be told, some time, just what it is that they might be in the

business of disconfirming vis-á-vis algorithmic processing. The misgiving is that an inability to state (or a deferral of the statement of) what amounts to the truth-conditions for a theory (the criteria of identity for the theory's constructs) is compatible with there not being anything in fact to which the theory corresponds vis-à-vis a mind/brain internal structure. The problem with this deferral, on the grounds given, is that it makes the (dis)-confirmation of the theory depend on a promissory psychology/neuroscience. And the problem with such a science is that it is indistinguishable from a fictional one. Conversely, if confirmation of the psychological reality of competence theories *qua* psychological entities requires a promissory/fictional science, then does this not also confer a no more than fictional status on mentally represented competence theories?

3.3 Choosing between grammars II: realism à la Dr. Pangloss.

The observation of the insulation of competence theories from psycholinguistic disconfirmation provokes, because it can not remove, the suspicion that their putative mind internal, causal role may be no more than "as if", no more than an instrumental, psychological fiction; fictions, of course, being somewhat resistant to empirical confirmation. However, this fictionality need not infect the notion of a competence grammar *per se*. Either one can maintain the psychological claim by providing further psychologically appropriate criteria or one needs a different, non-mentalist ontological interpretation; one can maintain a realism, just drop the psychologism.[87] This is, I take it, more or less Carr's position, and which position he takes for fairly well the same reasons as discussed here. His autonomism is existentially committed to linguistic objects which are taken to be abstract, mind-external and mind-independent. In sum, what we have is an E-language approach.

As remarked previously, where an E-language perspective is taken, where the object of study is a set of sentences:

> the linguist is free to select the grammar one way or another as long as it correctly identifies the E-language. Apart from this consideration, questions of truth and falsity do not arise.[88r]

By implication, from the I-language perspective there are truth-issues involved in respect of a decision between extensionally equivalent grammars. This constitutes a substantive distinction between the two conceptions of the linguistic, a distinction that affects the role of the formal systems that articulate linguistic theories. For the E-linguist, the calculus need only be interpreted in respect of the objects it picks out, i.e. the set of sentences that are generated (assuming a generative grammar). The differences between grammars that generate the same set of sentences are of no account in respect of truth. A grammar can be regarded as a more or less efficient instrument employed to the purpose of generating all and only the sentences of a language. The theorist's existential commitment need extend no further than quantifying over this set of sentences. This is in contradistinction to the I-linguist for whom there are determinate facts of the matter in respect of a grammar's economy of information.

As was remarked in chapter III, in the case of E-language approaches, while questions of truth and falsity do not enter into the choice between extensionally equivalent grammars, this does not remove the desirability of effecting a choice between competing, extensionally equivalent grammars. However, in this case the relevant criteria are concerned with issues of practicality, simplicity and economy. Whereas the truth concerns of an E-language perspective do not extend beyond attaining coverage, the I-linguist is concerned with "the correctness of the principles employed" and is so because, putatively, there are mind-internal facts of the matter to which the linguist's grammar is to correspond. Ostensibly, a methodology that would be appropriate to an E-language perspective needs to be supplemented by further criteria such as are relevant to the task of ascertaining the economy of information that is mind-internally represented, i.e. relevant to the empirical task of fixing the content of the mental representation. To this end Chomsky has proposed a series of increasingly stringent levels of adequacy. Grammars are required to meet the most stringent of these levels in order to warrant the appellation "psychologically real".

The minimal requirement is that a grammar be "observationally adequate".[89r] An observationally adequate grammar decides (and/or predicts) for any putative sentence its inclusion in, or exclusion from, the set of sentences constituting the language. A grammar meets this criterion of adequacy just in case all and only the members of the set of sentences of a language can be predicted/decided to be members of that set. Alternatively, one may think of an observationally adequate grammar as a function which enumerates the members of the set of sentences of a language. As we have observed, observational adequacy is sufficient to a linguistic

theory that (to conflate the Chomsky 1964 with that of 1986) takes E-language as its object of study. In fulfilling this project there need be no psychological (in the mind-internal sense of the term) interpretation of the grammar.

A psychological interpretation, on the other hand, requires that grammars are "descriptively adequate", that is that the grammar provides structural descriptions of strings that account for the intuitions that speaker-hearers of a language have about those strings. The example Chomsky gives is of ambiguous strings.[90r] The distinction he is making is between a grammar that simply characterizes a string as well-formed, with no regard to any internal structure, and a grammar that gives a structural description of the string (in the case of ambiguity, structural descriptions) which structural description accounts for and corresponds to the available intuitions a subject may have about that string, in this case the two (or more) available readings.

As remarked already, if one grammar meets this criterion, then there will be an indefinitely large set of other extensionally equivalent grammars that also meet it. To allow for discrimination between members of that set Chomsky proposes a further level of adequacy, which he dubs that of explanatory adequacy.[91r] A grammar is explanatorily adequate when it allows a reasoned choice of one descriptively adequate grammar over another "on the basis of an *empirical* hypothesis concerning the innate predisposition of the child to develop a certain kind of theory to deal with the evidence presented to him"[92r] (my italics). The claim here is that given the paucity of primary linguistic data and the rapidity with which language is acquired, then this is to be explained by a rich, innate, grammatical endowment which constitutes a template for the grammar that is to be acquired. This template ("Universal Grammar" or "UG") constrains the hypotheses of the child (viewed as a type of unconscious linguist) in the face of the data that he/she receives as input. In respect of the child, the initial state of the UG constrains the type of hypotheses possible. In respect of the linguist, competing hypotheses are decided between on the basis of an "evaluation metric" which selects for the simplest hypothesis; "simplest" being defined in relation to the economy of information of the initial state of the template. The UG provides the principles and the child, on the basis of the data available, chooses between the possible settings of the parameters of these principles: given the data, given the principles, the grammar selected for is the simplest. This process is, *mutatis mutandis*, essentially what the linguist recapitulates in attempting to discover the mentally represented grammar.

The model that is set up is that of a maximal relation between the UG and the final state of the system such that the UG is sufficiently restrictive so as to allow for

the similarities between natural languages while it is also sufficiently permissive as to allow for the differences. The UG is also required to be sufficiently richly specified so as to explain the speed of first language acquisition: more or less explicitly, the standard line is that this process is so rapid that the bridging of the gap between UG and the final state will require the minimum amount of work: UG contains the principles, it is simply the switch settings which need to be set. In short, there is a maximally economic relation between UG and the grammars of natural languages.

The crux of the observation to be made here is that it is not obvious to what, if any, extent these levels of adequacy are distinct in content from the application of purely methodological, non-empirical criteria of simplicity and economy, i.e. precisely the same criteria that the E-linguist has recourse to, but which, in the E-language case are invoked for no more than pragmatic, non-truth-relevant, instrumental reasons. Hutton observes the same: "Lacking external criteria for verification, Chomsky opted for simplicity as the criterion of explanatory adequacy."[93r] This is problematic. Simplicity is a perfectly proper *methodological* criterion: it allows one to constrain hypotheses and, all things being equal, one prefers the more parsimonious theory because it makes the fewer commitments and so fewer guesses; it is easier to test. But, then one tests it in respect of how the world, external to the theory and its method of construction, in fact turns out to be: the ability to construct a maximally economic theory is not itself the test of the theory's truth. Simplicity is, quite simply, not a truth-relevant criterion, not unless one assumes that the world, or the bit of the world one is interested in, is designed on maximally simple lines or, as is equivalent, that the Deity's method of world construction and His conceptual apparatus parallels one's own method of theory construction and the conceptualization in which it is figured - a distinctly Galilean assumption. In brief, to identify:

> simplicity as a marker for *truth* ... makes sense only if one believes that nature *is* simple, and will appear so through the filter of theory and language.[94r]

Yet, it is precisely as a marker for truth, as a criterion for a realism, that the set of levels of adequacy is canvassed.

These criteria are intended for the purpose of winnowing down the number of extensionally equivalent grammars such that the grammar selected has the best claims to correspond to the economy of information of the postulated MRG (i.e. correspond

to, that is, to be *true* of the putatively mind/brain-internal facts of the matter). Lacking the external criteria that would test the answerability of hypotheses to how the mind/brain turns out to be, this procedure only makes sense if purely methodological and theory-internal considerations can be taken to be sufficient to the making of a truth claim. Such considerations are sufficient only on the assumption that the mind/brain, an evolved and contingent structure (contingent for, presumably, it could have turned out otherwise, e.g. more or less complex), nevertheless evidences not just some certain simplicity in design, but a simplicity that is capturable in, and commensurable with, the language, the conceptual apparatus, that is used for expression of theory.

As Chomsky reminds us simplicity is relative to particular theories, it is not an absolute. The assumption that theory-internal and methodological considerations may serve as directly truth-relevant leads to the transposition of those considerations and the correlative projection of the terms of the conceptual apparatus (Wittgenstein's "spectacles") that articulates theory, and in respect of which apparatus simplicity is defined, onto the world as, in the former case, real principles and, in the latter case, real objects. But the warrant for their reality is only as good as the warrant for the making of the assumption and, as Katz points out, there is no reason to exclude the possibility that "the human mind is so constructed that its representations of grammatical knowledge use more theoretical apparatus than is necessary".[95] If this is so, then parsimony cannot, on the mentalist construal of the linguistic, be a relevant criterion to decide between theories in respect of their correspondence to any psychological facts of the matter.

The outcome is a study that becomes "not a 'direct' study of the domain itself, but rather a study of [the conceptual apparatus that articulates] our knowledge of it."[96r] Moreover, what one might predict to be the upshot of the making of this assumption, (which, insofar as it is unwarranted, results in no more than "a heuristic hocus-pocus"[97r]) is, in fact, what one gets: a theory, Minimalism, which, when construed in a realist light, predicates of the mind/brain a language faculty that evidences a certain "perfection" in design and which conforms to "virtual conceptual necessity".[98r] This is just what one would expect where a theory's construction, lacking theory-external criteria, applies to theory-internal considerations and is run off the conceptualization that is applied to the task of figuring the phenomenon.

Oddly enough, it is just the lack of interest in external validation (duplicated, in the Chomskyan case, by lack of availability of such criteria), with a correlative lack of interest in the way the world, as opposed to our concepts, happens to be that is the

stick which Chomsky uses to beat the Platonists. Even more oddly, even the phrase is the same:

> attempts to capture those properties of language that are logically or conceptually necessary [is no more than] an enquiry into the concept 'language'. [Furthermore, this enquiry is] unlikely to prove more interesting than an enquiry into the concept 'vision' or 'locomotion'.[99r]

While it is the case that this is not what the Chomskyan psychologistic project was in attempt of, that the result is a theory that posits conceptual necessity might suggest that, in substance, there is nothing to distinguish the mentalist and the Platonist/autonomist perspectives, which is to say that the psychological facts of the matter that answer to Minimalism's perfections are indistinguishable from fictions. In brief, there are grounds to suspect that a theory that concludes in conceptual necessity is unlikely to prove more than a skeuomorph, or, as Chomsky has it:

> a kind of artefact, a result of our methods of investigation and theory construction, not properties of the real object of the real world that we are investigating.[100r]

These non-empirical considerations aside, what we also get from a theory that predicates "perfection" of an evolved, biological system is "a problem, possibly even a mystery", for one is led to ask "why language should be so different from other biological systems."[101r] Chomsky takes linguistics to be, ultimately, a branch of biology. Consequently, Minimalism is, supposedly, making a substantive biological claim. The difficulty is that, although at last finding some theory-external considerations in respect of a domain that is claimed to be relevant, what those considerations tend towards is the refutation of a theory that predicates "perfection" of a biological system, for, as Chomsky himself remarks, evolved, biological systems are typically not like this at all.

However, perhaps this perfection is indeed the case, perhaps Minimalism's conclusions constitute a problem for biology and not for linguistics. For, after all, what we are in account of are similarly theory-(*qua* calculus)-external considerations, i.e. the facts of language acquisition. Amongst these facts, and what we are encouraged to be impressed by, is the speed of first language acquisition. It is this fact that suggests the need for a maximally economic relation between UG and the final

state grammar and so the taking of simplicity as a truth-relevant, as opposed to merely a methodological, criterion.

Chomsky more or less recapitulates this point in remarking that the Minimalist Program is the culmination of an attempt to resolve the tension between the task of achieving a descriptively adequate grammar and that of attaining explanatory adequacy:

> [The] central objective [of the research program] was to abstract general principles from the complex rule systems devised for particular languages. ... Steps in this direction reduce the range of language-specific constraints, thus contributing to explanatory adequacy. They also tend to yield simpler and more natural theories, laying the groundwork for an eventual minimalist approach.[102r]

And what makes truth-relevant sense of this "abstraction of general principles" is the fact of the speed of first language acquisition. A fact that is wheeled out as a standard platitude in innumerable textbooks. To remind ourselves: within a period of approximately three years an infant moves from virtually zero ability, in production at least, to more or less a complete command of the structural diversity of the language and, barring pathological cases, this ease of acquisition is universal, seemingly being uncorrelated with any other possibly relevant factors such as IQ. So, the argument goes, this ability must require a phenomenal speed and efficiency on behalf of the language faculty.

But, what precisely do we mean by "speed" here, and "speed" relative to what and to what that is relevant? In raising the rhetorical question it is difficult to think of even a rhetorical reply. If we grant that talking of speed is not entirely vacuous, then we need to ask whether this speed is maximal, as fast as it possibly could be. The trouble is that it is not clear at all what we could mean by maximal speed, and nor what possible circumstances, if we make the claim, could falsify it. Indeed, it is much easier to concoct arguments to the effect that language acquisition is not as fast as it could be in some possible world. For instance, it is well-known that children over-generalize their "hypotheses" and stubbornly persist in them despite plenty of disconfirming evidence that, it seems, it takes them time to assimilate. One could reasonably say that this is disconfirmatory of the claim that acquisition is maximally fast. The rejoinder might be that, given the mechanisms that are involved, which mechanisms are as fast as they are, then acquisition is as fast as it could be. But this is

just saying that acquisition is as fast, and as slow, as, in fact, it is, no more and no less. To persist with the claim that acquisition is maximally fast appears to require a distinctly Panglossian frame of mind: acquisition is maximally fast because this is the fastest (and simplest) of all possible worlds. And this is the route that is taken: one opts for simplicity as a quasi-metaphysical principle; simplicity as a marker for truth. There is, however, simply no warrant for this. Of course, there is the need for such a principle in *lieu* of external criteria, for otherwise one's psychological realist claims for one's theory would be embarrassed by the complete inability to distinguish between competing, extensionally equivalent grammars. This brings us back to the Quinean stricture: for theoretical constructs that one cannot truth-relevantly tell the difference between, one cannot contentfully claim there to be any (I-language) facts of the matter to which the constructs answer. What we also appear to get is corroboration of the interpretation of Quine's (and Searle's) arguments as non-*a priori* and predictive of the failure to achieve such content.

Once one takes the "fact" of the speed of acquisition away, it appears that there are no other facts to which the perfection of the linguistic system answers. Indeed, we are left with only the counter-facts provided by evolutionary biology: predicating perfection of an evolved system, as we canvassed earlier, constitutes a virtual *reductio ad absurdum* of the biological credentials of the theory that makes the claim. Quite simply, evolved systems are, for the most part, rather messy and are so by virtue of being evolved. They do not evidence the work of a creator with a design award in mind, nor do they provide examples of maximal design efficiency. Physiological, and presumably psychological, structures and faculties are arrived at, we are told, through a blind process of chance mutations, for the most part false starts, which get selected for on the basis of the advantages they provide for the organism. The whole business is not conducted on the basis of an end in view, consequently the successful organisms that populate the world carry with them a record of the false starts and changes in direction. That is why we have redundant and non-optimal body parts, tonsils and appendices and the like. This is also presumably why, according to Wolpert, that "often in the history of biology neither the simplest nor the most elegant theory [has] turned out to be right."[103r] Moreover, simple systems are economical systems with correspondingly minimum waste, redundant effort and superfluity. The trouble with such systems is that they crash at the slightest provocation; damage to one element and the whole becomes unusable. This does not seem to be the case with evolved systems for the simple reason that such fragility would leave the organism open to the possibility that the slightest damage would result

in the loss of the relevant function. In this respect, a certain redundancy in design is a distinct advantage for the organism, allowing it to function even when partially damaged. This appears to be the case with the human language faculty: severe lesions are rarely so severe as to result in the total collapse of the system. Furthermore, the common recovery of full, or nearly full, functioning indicates that the mind/brain is capable of a considerable degree of plasticity through re-dedication of brain areas from one function to another.[104r]

In this light, it seems that Chomsky was indeed correct to point out that Minimalism's conclusions constitute a problem, the problem is that it appears to refute itself as a theory of a psychological and biological system. At this point there are, logically, two options: something is wrong somewhere and it is either in the biological sciences or in linguistics. Chomsky opts for the former.

One strategy Chomsky takes, as we have previously noted (chapter III, 4.4), is to cast doubt on the relevance of evolutionary accounts in respect of the human language faculty and so strand, by stipulation (and self-serving necessity), linguistic theory from such (dis)-confirming evidence:

> It is perfectly safe to attribute this development [of mind-internal, innate knowledge of language] to "natural selection", so long as we realize that there is no substance to this assertion, that it amounts to nothing more than a belief that there is some naturalistic explanation for these phenomena.[105r]

An alternative tack is to play the ball resolutely into the biologists side of the court and suggest that there must be something wrong with biology such that it has lamentably failed to uncover certain underlying principles that organize biological systems and if the biologists would only get cleverer all would be well.[106] Either that or invoke "epistemic boundedness" and put the issue in the box marked "mysteries" - that's what you put in, what you get out is irrefutability.

One might wonder what warrants Chomsky in his confidence that the problems raised are, indeed, problems for the other sciences. The answer, it seems, is the productivity in the explanations afforded, what we have referred to as "heuristic fertility". The trouble with this is that the maintenance of a realism towards the linguist's conclusions infects the enterprise with considerable heuristic *in*fertility: it does not so much explain as mystify. For, do we not need some account as to why the linguistic faculty should be so uniquely and anomalously special as to be outside of an

evolutionary explanation? And is it not at least of questionable rationality to maintain a position that depends on either the incorrectness of theories in other domains (which theories have some firm foundation and their own explanatory success), and/or on some unique anomaly?

In general terms, what is going on here is that, as we have had cause to emphasize, Chomsky's reductionist psychologist project claims the relevance of evidence from adjacent domains of study. This is what makes the reductionist psychologism cogent. When such evidence is disconfirmatory, where it is available at all, then, on the grounds of the explanatory success of the linguistic theory, one gets to insist the problem lies in the apparently disconfirmatory evidence, or in the fields which, if they would only get themselves properly developed, would furnish proper, corroborating evidence; as if a criterion of their success were their having to confirm linguistic theory, and as if it is this latter which must (on the basis of some, have we not agreed, illegitimate notion of conceptual necessity?) be right. What results is the present failure to give any content to one's reductionist psychological claims and, correlatively, something that looks remarkably like unfalsifiability from the perspective of these supposedly evidentially relevant domains.

The problem that the criterion of explanatory adequacy was designed to address was that of winnowing down the number of extensionally equivalent grammars and winnowing them down in respect of empirical truth. What might have warranted simplicity as a truth-relevant criterion was the "speed" of acquisition. Once this "fact" is undermined, then also undermined is the truth-relevance of choosing between grammars in respect of their comparative simplicity. The upshot is that all grammars that are extensional equivalents of the most constrained theory are equally heuristically fertile. There are grounds to select between them, but the only available grounds are considerations that are relevant to the assessment of grammars *qua* systems of notation, i.e. as instruments that are more or less efficient, or elegant, or admit of greater ease of use. Exchanging one grammar for an extensional equivalent raises no truth issues. But, where there are no truth issues, then neither are there facts of the matter. At least, what is lacking are proper and available criteria of identity for the postulated MRG, and, to mix philosophers (like one mixes metaphors), whereof we cannot adduce criteria of identity, thereof we cannot credit existence.

This outcome is somewhat paradoxical: Chomsky's psychological realism "led to the choice of system-internal (or notation-internal) measures as criteria for distinguishing [between hypotheses]. [Psychological] realism led in effect to a

heuristic hocus-pocus."[107r] The paradox derives from the tensions that result from the inability to show the correspondence with some mind internal state of affairs on the one hand, and, on the other, the insistence that such a mind internal state of affairs is causally explanatorily powerful. The inability to show any very direct correspondence between the particular economy of information of the most constrained grammar and that putatively instantiated in a MRG results in an inability to validate the mind-internal, psychological and so causal explanatory status of a competence grammar. Without such validation, a competence grammar has no substantiated psychological locus, it is, at optimistic best, temporarily autonomous of the psychological.

The paradox dissolves once one drops the psychologism with its correlative commitments to a MRG and construal of the object of study as I-language. What we then get is a position broadly in line with Carr's autonomism (and/or Katz' Platonism). The inability to adduce any facts of the matter in respect of any differences between extensionally equivalent grammars only undermines the psychological reality of the grammar, not the reality of the set of sentences a grammar it is in account of. What we also get is a constitutive explanation, which is to say, what we do not get, and are seemingly precluded from, is an explanation of the relation between the linguistic and individual speakers.

What is so unappealing about an autonomism/Platonism is just this anomalous inability to furnish an explanation, this together with the justification for this inability only through recourse to further mysteries. Such considerations provide a motive to avoid going this direction. Perhaps we can still breathe some life into Chomsky's reductionist psychologism.

4.0 Inference to the best explanation.

The way towards maintaining a psychological ontology for the linguistic, and the way that, interpretably, Chomsky encourages us to take, is indicated by pointing to the explanatory power of the mental representation hypothesis as separable (justified by the necessary "abstraction" of the enquiry) from any particular content; it is the best and only explanation on offer and, invoking Harman's notion of the inference to the best explanation, this fact constitutes grounds for its acceptance:

> Suppose that our most successful mode of explanation and description attributes to Jones an initial and attained state including rules ... and explains Jones' behavior in these terms; that is, the rules form a central part of the best account of his use and understanding of language and are directly and crucially invoked in explaining it in the best theory we can devise. ... I cannot see that anything is involved in attributing causal efficacy to rules beyond the claim that these rules are constituent elements of the states postulated in an explanatory theory of behavior and enter into our best account of his behavior.[108r]

Furthermore, (Chomsky's interpolation):

> As Demopoulos and Matthews observe, "the apparent theoretical indispensability of appeals to grammatically characterized internal states in the explanation of linguistic behavior is surely the best sort of reason for attributing to these states [and, we may add, to their relevant constituent elements] a causal role in the production of behavior."[109r]

In sum, the explanation cannot do without mental representations and the provision of an explanation is itself the confirmatory evidence. What comes out of this is that the interpretation of the enterprise subtly shifts: the MRG hypothesis becomes evidentially distinct and separable from the task of adducing the content of the competence grammar. Indeed, it needs to be so because the claimed for necessary abstraction of the linguist's discourse constitutes a breaking of contact from the putative actualities of the economy of information of a mentally instantiated grammar.

This seems to be the way to make sense of Chomsky's suggestion that, in the case of extensionally equivalent formulations, the problem is to be resolved by "find[ing] a more abstract principle ... incorporating just the relevant features shared [by equivalent grammars] without the extra structure that differentiates [them]."[110r] Given our previous remarks to the effect that there are no grounds to suppose the mind/brain is designed on the lines of maximal economy, then there are no truth/mind-internal-relevant reasons to prefer this "more abstract" formulation. There are no reasons to suppose that there is anything wrong (not true) about either of the original formulations, only that the more abstract is more efficient in capturing significant generalizations.

What results from this reappraisal of the enterprise is that the linguist's grammar is, in effect, in account of what is known when we know a language, the object of knowledge, as opposed to an account of the internally realized economy of information which is, at the level of information, *how* it is known. This translates into the idiom of cognitivism as an account of the level one function computed. It is a case of substituting the word "functional" for the word "mental" and making an act of faith that some future enquiry will reassimilate the two terms. Provisionally, and only provisionally, this constitutes a shift from "psychological" in a contentful mind/brain internal sense to "psychological" in a weaker, object-of-a-propositional-attitude sense, where the mind/brain is not shown to be directly constructive of the nature of the phenomenon, rather this relation, and so the psychological ontological status of the linguistic, is more of an act of faith, a faith based on the observation that what the MRG hypothesis provides is an explanation by way of a naturalizing and psychological ontological locus, and what we need is an explanation. This is, of course, simply autonomism, or perhaps an ontological indeterminacy, supplemented by the promissory, i.e. an autonomism by another name (and one which, perhaps, smells the sweeter to standard prejudices).

The psychological, non-autonomous ontological status for the linguistic is maintained, and the slide to an autonomism halted, by adverting to, on the one hand, some future validation by the brain sciences and, on the other and in the meantime, to the explanatory success of the mental representation hypothesis. It is the explanatory power of this thesis in offering an account of acquisition, language variation within distinct parameters and, in general, a response to the question as to why the natural linguistic is as it is which constitutes the grounds for the status of the MRG hypothesis as best theory. It is just this ability to explain that constitutes the warrant, à la Harman, for accepting the explanation.

However, the further move that Chomsky encourages us make is to accept the truth of the explanation, to take up a realism towards MRGs, this being the "social practice" in respect of best theories; in short, to equate acceptance with belief (the philosophers remind us that to believe x is to take x to be true). This is, however, to neglect a distinction:[111r] there need be no strict correlation between the set of propositions one may happen to accept and the set of propositions one holds as true. For example, when buying a lottery ticket it is rational to accept that the ticket will not win, but one does not thereby also hold the *belief* that it will not win. If one did, it would be wholly irrational to buy lottery tickets. Similarly, one might accept and rationally so, on the basis of its accuracy and simplicity, the explanation that a coin

which falls heads 550 times out of a 1000 is biased to give a probability of heads of 0·55. However, one might question the truth of this explanation, without impugning its explanatory success, by considering the antecedent probability of such a bias. (Notice that what these remarks hint towards and are consistent with, is an instrumentalist, as opposed to a realist, view of an accepted theory.)

Our discussion has considered the theory-constructive role of the antecedent conceptual apparatus that is the vehicle for hypotheses in the generative paradigm. In doing so we have identified certain tensions, which we dubbed skeuomorph effects, that arise out of a realist construal of the constructs modelled by the formal systems that articulate theory. This was not to deny the heuristic fertility of the generative paradigm - and so is not to deny the grounds for acceptance of that paradigm - but it is to raise the question of the antecedent probability of, and so grounds for belief in, the truth/reality of that explanation and, by a certain transitivity, the reality of the postulated object of study, the I-language. In short, the suggestion being investigated is that the explanation provided is no more (and no less) than an instrumental fiction. Our concern in the following chapter is to address the question of the explanatory and ontological status of level one functions and whether they can be accorded any more than an "as if" psychological status, indeed, any more than a feigned and "as if" existence.

Footnotes.

[1] Except in so far as certain constants of the mathematical expressions are specified as referring to the appropriate entities. We ignore here the question as to whether laws are themselves taken to be abstract entities.

[2] N. Chomsky, (1986, pg. 39).

[3] The need to reflect that economy of information is behind Chomsky's positing of levels of adequacy (N. Chomsky, (1964, section 2 and 1965, pgs. 30 - 38)).

[4] Examples are T. Nagel (1969), R. Schwartz (1969), W. V. O. Quine (1972), J. Fodor (1983), J. Searle (1980) and M. Dummett (1981). Indeed, the point is anticipated by Wittgenstein:

> In philosophy we often *compare* the use of words with games and calculi which have fixed rules, but cannot say that someone who is using language ***must*** be playing such a game. (1958, section 81).

[5] N. Chomsky (1965, pg. 9).

[6] J. Searle, (1980, pg 37).

[7] J. Ullman, (1979).

[8] R. Berwick, (1991, pgs. 137 - 138).

[9] I am aware that this is hardly uncontroversial and also raises issues that are beyond the present remit. One might be tempted to the view of such laws, the referents of the formalism, as real. If they are real, then there should be criteria of identity. The only criterion of identity that seems unproblematic concerns the behaviour that the laws predict, which is, as it were, the laws' extensions. Laws also appear to have an ineliminably intentional aspect, and, of course, intentionality is a mark of the mental. So, the question, which I will leave moot but with a bias towards a negative response, is that of whether laws exist independently of minds (the mark of realism)? This is distinct from the question of whether the behaviour would exist if there were no minds to observe it.

[10] S. Stich (1985, pg. 143).

[11] It may also be remarked that the intentionality of the system, (i.e. what it is about,) in the case of a calculator, as with any computer program, is external to the system. The calculator only becomes a machine that performs mathematical functions when its user interprets the input and output of the calculator in the appropriate way. Fodor makes a similar remark in respect of computer programs: it is possible that the same program could be used to calculate the batting averages of a baseball team or the gross national product of Monaco, (J. A. Fodor, 1980). Also see R. Cummins, (1989).

[12] G. Harman, (1965).

[13] See J. Searle, (1992, pg.245 - 246): Searle points to the dependence on a set of intentional rules as the manner of explanation, but these rules never achieve more than confirmation in the style of "it is

as-if [an individual] were following [these] rules." "The problem with *as-if* intentionality is [that] its identification does not give a causal explanation."

[14] Compare Chomsky, (1986, pg. 5).

[15] J. Searle, *opus cited*, pg 241.

[16] A behaviourist and a Platonist linguistics both share the view that the linguistic is not to be explained in terms of some set of "inner causes", although for rather different reasons; in the former case the reasons are primarily methodological (to do with verification), in the latter case, ontological.

[17] W. Demopoulos and R. Matthews, (1983, pg. 406). Cited in G. Adriaens, (1986, pg. 60).

[18] J. A. Fodor, (1985, pg. 153).

[19] W. V. O. Quine, (1972) and J. Searle, (1992, pgs. 150 - 162).

[20] The phrase is Quine's (W. V. O. Quine, 1972, pg. 445).

[21] W. V. O. Quine (1960).

[22] See, for example, W. Demopoulos and R. Matthews, (1983), A. George, (1986), G. Evans, (1985), M. Davies (1989) and H. Putnam, (1989). These in addition to Chomsky's rebuttals, (1980 and 1986).

[23] This example, the origin of which, by the way, comes from a misreading of Hegel's doctoral thesis, is borrowed from J. A. Fodor (1981).

[24] W. V. O. Quine (1979, pg.244). This is consistent with George's (1986) discussion of the Quine/Chomsky debate which he interprets as, at root, no more than a disagreement about the directions in which cognitive psychology is likely to take.

[25] M. Davies, (1989, pg. 132), the same point is made by Evans, (G. Evans, 1985).

[26] W. V. O. Quine, (1972, pg. 447).

[27] J. Searle, (1992, pg. 152).

[28] *Ibid.* pg. 158.

[29] *Ibid.* pg. 159.

[30] A question to which we will return is that of the extent to which Chomsky's talk of the level of a theory's discourse being "in abstraction from whatever may turn out to be the mechanisms that account for these properties" (N. Chomsky, 1986, pg. 23) is, oddly, compatible with Searle's position. Perhaps the system of intentional rules may be regarded as introduced (a skeuomorph?) as part of the abstraction of the enquiry and of the conceptual apparatus that introduces the discourse. Perhaps "talk of mental and abstract structures is merely an inadequate [and temporary] heuristic in the long term goal of finding the physical reality behind them," and that, furthermore, the theory of this physical reality will replace the terms of the linguist's discourse. The quotation is from Hutton (1990, pg. 116) who, wrongly I think, interprets Chomsky to be party to this eliminative reductionist and so instrumentalist view. Wrongly, because it does not make best sense of Chomsky's quite explicit realist pronouncements. On the other hand, as we are concerned with showing, such an instrumentalist

construal does make best sense of precisely the position we are reduced to by the failure to achieve any content to the promissory (non-eliminative) reductionism.

[31] P. Churchland and T. Sejnowski, (1990, pg. 226).

[32] J. A. Fodor, (1985, pg. 153). Quinean in the sense that a thread which runs through Quine's writings is the insistence that philosophy is not something which is prior to, and normative of, the scientific enterprise.

[33] N. Chomsky, (1980, pg. 220).

[34] N. Chomsky, (1980, pg. 220 and 1986, pg. 23).

[35] A. George, (1989, pg. 94).

[36] R. Botha, (1979, pg. 45). Similar observations are made by Hutton (C. Hutton, 1990, pg. 113).

[37] This psychogrammar/grammar distinction is hard to square with Chomsky's contention that "Language has no objective existence apart from its mental representation" (N. Chomsky, 1972, pg. 169).

[38] Z. Pylyshyn, (1991, pg 240).

[39] E. Stabler, (1983). (Compare chapter I, footnote 78.)

[40] N. Chomsky, (1980b, pg. 54.)

[41] N. Chomsky, (1986, pg. 46).

[42] N. Chomsky, (1965, pg.9).

[43] N. Chomsky, (1986, pg. 222).

[44] N. Chomsky, (1980, pg. 11).

[45] E. Stabler, *opus cited*, pg. 401.

[46] J. A. Fodor, (1985, pg. 153). One may equally say: take the notion of mental representation away and you take away the means to get a sort-of-causal/reductionist explanation out of the calculus.

[47] N. Chomsky, (1986, pg. 40).

[48] G. Miller and N. Chomsky, (1963, pg 481).

[49] See J. A. Fodor *et al.*, (1974).

[50] J. D. Fodor, (1991, pg. 84).

[51] N. Chomsky, (1967, pg. 435 - 436).

[52] N. Chomsky, (1965, pg. 9).

[53] J. Bresnan and R. Kaplan, (1982, pg. xxxvii).

[54] D. Steinberg, (1993).

[55] J. D. Fodor, (1991, pg. 86).

[56] Note how much of the psycholinguistic literature is taken up with arguing the toss over the interpretation of the experimental findings. Moreover, given that the measurements made are standardly of processing time or eye movements, then the findings are not terribly fine-grained: they can suggest that sentence A is rather more difficult than sentence B and even, from eye movements,

where the processing difficulty lies in some string, but even then it is necessary to invoke a series of plausible (but not incontrovertible) assumptions about, for instance, the correlation of difficulty/complexity with processing speed. A further reason to demur is the emphasis (out of practicality) on the measurement of subjects' performance in reading. Certainly, it is not implausible that reading directly implements and so gives transparent access to wholly linguistic processes, but then again nor is it implausible that reading, a learnt and non-natural skill, is a distinct cognitive activity which, while, presumably, accessing purely linguistic abilities, may also be, to some extent, ***sui generis***. Consider the phenomenon of dyslexia, for instance.

[57] It is, reportedly, common practice to term a theoretical construct "'psychologically real' [only] if it plays a particular kind of role in a particular kind of experimental procedure." (K. Wexler and P. Culicover, 1980, pg. 44, cited in F. Newmeyer, 1983, pg. 43).

[58] Pylyshyn makes a pertinent point in remarking: "It is a strange fact about the field of psychology, that whenever psychologists do meta-science they appear inevitably to revert to behaviorism." (Z. Pylyhsyn, 1991, pg. 249, fn. 3.)

[59] S. Crain and J. D. Fodor, (1985, pg. 95).

[60] J. D. Fodor, (1991, pg 86).

[61] R. Berwick, (1991, pg. 119).

[62] T. Winograd, (1983, pg.89). This is not to say that, methodologically, it is not the best, because simplest, assumption. But one cannot, of course, argue from method to how things are in fact, not unless one can adduce grounds for why things should conform to the canons of methodology.

[63] N. Chomsky, (1982, pg. 33 -34).

[64] N. Chomsky, (1980, pg. 89).

[65] G. Carlson and M. Tanenhaus, (1982, pg. 57 - 58), cited in F. Newmeyer, (1983, pg. 47).

[66] L. Wittgenstein, (1967), cited in N. Chomsky, (1980, pg. 49).

Chomsky's position here is not, in result, dissimilar to Davidson's "anomalous monism", (D. Davidson, 1980). The essence of this view is that the description of the mental (linguistic) is irreducible to any physical description; while the mental is supervenient on the physical, it is not possible to derive mental laws from a description of the physical substrate. The upshot is that even if there were a completed neuroscience, there would still be no completed psychology.

Nor, as might have been noticed, is there much, in respect of hard content, that distinguishes this position from Popper's.

[67] The same observation is made by J. Bresnan and R. Kaplan, (1982, pg. xxiii).

A correlative position, an ontological agnosticism, is taken by Gazdar *et al.*: "We make no claims, naturally enough, that our grammatical theory is *eo ipso* a psychological theory.... Thus we feel it is possible, and arguably proper, for a linguist (*qua* linguist) to ignore matters of psychology." (1985, pg. 5).

[68] The phrase is Peacocke's, (1989). Also, compare Bresnan's "realization problem" (1978).

[69] Z. Pylyshyn, (1991, pg. 234).

[70] C. Peacocke, (1986 and 1989).

[71] N. Chomsky, (1965, pgs. 30 - 38).

[72] D. Marr, (1982). Marr's field was the human visual system. The hierarchical framework developed in *Vision* was largely in response to the lack of success in developing an account through an enquiry into neurophysiological mechanisms. He explicitly takes Chomskyan competence theories as exemplars of computational theories of the level one function computed (pgs. 28 - 29). Also compare Marr's level one to Newell's "knowledge level", (A. Newell, 1982) and, for an early precursor, E. Adrian, (1954).

One consequence of Marr's influence in mainstream AI has been, arguably, and pertinently in respect of our discussion, a breaking of contact between, in Hofstadter's words (1985, pg. 639), some "vision of how the *mind* works ... and speculation about how the *brain* works."

[73] N. Chomsky, (1986, pg. 5).

[74] D. Marr, *opus cited*, pg. 29.

[75] N. Chomsky, (1980, pg. 89). For an uncannily accurate parallel between the Chomsky/Marr and the Popperian position in this particular regard consider (my italics):

> Few things are as important as the awareness of the distinction between the two categories of problems: production problems on the one hand and problems connected with the structures produced on the other. ... The problems of the second category are basic for understanding the production problems: contrary to first impressions, we can learn more about production problems by studying the products themselves than we can learn about the products by studying production behaviour. This ... thesis can be described as an anti-behaviouristic and *anti-psychologistic thesis*. (K. Popper, 1972, pgs. 113 - 114, cited in P. Carr, (1990, pg. 41).)

Here "anti-psychologistic" in the sense of a psychology defined as the study of mental processes. Popper's use of the epithet does, however, raise the question again of the ontological status of the objects that level one theories describe and whether they can be related to states or properties of the mind/brain in such a way as the mind/brain is constructive of the objects of level one theories.

[76] J. van de Koot, (1990, pg 14).

[77] C. Peacocke, (1989, pg. 117).

[78] *Ibid.* pg. 114.

[79] *Ibid.* pg. 112.

[80] *Ibid.*

[81] *Ibid.*

[82] A. George, (1989, pg. 101).

[83] N. Chomsky, (1986, pg. 33).

[84] Z. Pylyshyn, (1991, pg. 239).

[85] See N. Chomsky, (1986, pg. 23 and pgs. 256 -257).

[86] C. Peacocke, (1989, pg. 120).

[87] Alternatively, one can go George's direction (1989) and just widen the psychological domain to include the objects of propositional attitudes. The trouble with this is that it looses the whole point of Chomsky's psychologism, i.e. to explain why natural languages are as they are in terms of their relation to individuals.

[88] N. Chomsky, (1986, pg. 20).

[89] N. Chomsky, (1964, chapter 2).

[90] N. Chomsky (1965, section 4.)

[91] *Ibid.*

[92] *Ibid.*

[93] C. Hutton, (1993, pg. 175).

[94] This is quoted from the entry for "simplicity" in T. Honderich (ed.), (1995).

[95] J. Katz, (1985, pg. 197). Katz contrives to take this fact as constituting evidence against a psychological ontology for the linguistic on the basis that such an ontology commits us to "methodologically perverse choices" (pg. 198). This rather sounds like a claim that sciences do not have to answer to how the world turns out to be, as if we could conduct enquiries from our armchairs and simply apply our methodology. It would also allow us to conclude that where the world, for example, is more complex than it really need be, then the world must have got it wrong.

[96] A. Eddington, (1938, pg. 123), cited in P. Carr, (1990, pg. 68).

[97] C. Hutton, (1993, pg. 175).

[98] The phrases are Chomsky's, (1995a, e.g. pg. 389).

[99] N. Chomsky, (1980, pgs. 29 - 30).

[100] N. Chomsky, (1991, pg. 50). It needs to be added that Chomsky thinks that this is unlikely.

[101] N. Chomsky, (1991a, pg. 50).

[102] N. Chomsky, (1995, pg. 388).

[103] L. Wolpert (1998).

[104] Much the same points are made by Johnson and Lappin, (1997).

[105] N. Chomsky, (1972, pg. 97).

[106] I suspect that such claims would leave the average biologist in a state of apoplexy. Nonetheless, this position was publicly aired, by Chomsky, in a talk at London University (June, 1995). There were, I think, no biologists present. If you find the claim somewhat breath-taking in its, let us say,

contentiousness, and suspect that I might have misheard, then I was not the only one, see Johnson and Lappin (1997).

[107] C. Hutton (1993, pg. 175).

[108] N. Chomsky, (1986, pgs. 252 - 253).

[109] N. Chomsky, (1986, pg. 257), W. Demopoulos and R. Matthews, (1983, pgs. 405 - 406)

[110] N. Chomsky, (1986, pg. 255). This interpretative line is also taken by van de Koot, (1990, pg. 15 - 16).

[111] Compare S. Blackburn, (1994, pg. 131).

CHAPTER V.

Cognitivism and Functionalism.

1.0 Introduction.

The Chomskyan paradigm is, as we noted, in accord with mainstream cognitivism. In broad terms, this is the view that behaviour is to be explained in terms of mind/brain internal states (contra Behaviourism) and that the nature of these states needs recourse (contra Dualism) to neither ontological categories nor explanations that are at odds with an entirely scientifically respectable materialism (for are we not all good materialists now?[1]). The individual is to be usefully construed as an information processing system. This latter idea gets articulated and clarified by the mind-as-computer metaphor: individuals make use of mental representations in much the same way as computers make use of programs. Cognition becomes a matter of the rule-governed manipulation of representations. Correspondingly, the task for the cognitive scientist is one of determining the psychologically real programs that the mind/brain runs.

Placing the Chomskyan scheme within the more general cognitivist paradigm in turn aligns the Chomskyan scheme with the functionalism that provides cognitivism with its philosophical rationale. The essence of this position is reflected in Marr's hierarchy: the privileged level of explanation of cognitive functioning is that of the level one function computed. It is this function, in contradistinction to any algorithm or hardware, which is to be identified as the primary explanatory and psychological construct.

Our aim in the following is to argue that, while the Chomskyan enterprise is indeed in line with mainstream cognitivism (and so takes on to itself the credence which is, apparently, widely afforded to the cognitivist paradigm), however, an appeal to the authority of the cognitive enterprise does no more than result in a conclusion which, on the one hand, from the perspective of the mental representational hypothesis, encourages an instrumentalism, and, on the other hand, from the perspective of a functionalist rationale, recapitulates Carr's contention that the linguistic, defined as a level one theory, is to be properly construed as an ontologically autonomous domain. The options are then either to rest (not very) content with that conclusion - for after all, somewhere down the road there must be some study that

contentfully relates the linguistic to the individual subject in terms of individual psychology; a study which will possibly reconfigure our ontology and conceptual commitments - or, alternatively, one might bring to question the reality of the object of study that a level one theory addresses.

2.0 Cognitivism and Folk Psychology.

Implicit in the cognitivist enterprise is a realism toward the intentional content of level one theories. If cognitivism is to be in the business of providing real explanations of cognitive behaviour, then its central theoretical constructs, level one theories, have to correspond to the explanatory facts of the matter. The explanation provided by level one theories standardly applies to a claim that the causality of cognitive functioning is, in part, run off the intentional content of posited rules. The trick is to negotiate the intentional content of level one theories within the parameters of a prevailing materialism. The solution is the mental representation hypothesis in which the two sides, of what is essentially Descartes' dualism, are mediated: the intentionality of the posited rules and representations (intentionality being a (perhaps *the*) mark, of the mental[2]) is assimilated to a thorough-going materialism by means of the (neuro)-physical structures that are doing the representing. While the specific, presumably neurological, mechanisms that instantiate the representations are presently unknown, the problematic relation between the abstraction of the intentional content and the physicality of the structures doing the representing does not call to be negotiated by recourse to the postulation of some shadowy and mysterious immaterial mind-stuff. Mind-stuff, it is the contention (compare Chomsky), is no more than some fancy, as yet unknown, arrangement of ordinary matter.

2.1 Taking an intentional stance, taking a knowledge stance.

The pattern for cognitivism's explanations (i.e. by way of theoretical constructs packing intentional content) is the "theory" which we routinely use in explanation of one another's behaviour by ascribing beliefs, desires and hopes (i.e. propositional attitudes that are individuated by their intentional content) to other individuals. This "theory" being variously described as a "theory of mind", "the theory-theory", "intuitive psychology", or, the one we will stick with, "folk psychology" (FP).[3r]

Routinely we causally explain why Tom left in a hurry, or why Dick worked hard, by ascribing, to Tom, the belief that he would otherwise miss an appointment and, to Dick, the desire to get promotion. This "theory", in much the same way as cognitivist theories, posits and quantifies over intentional constructs (in this case, beliefs and desires etc.) that are taken to be involved in the aetiology of behaviour. The basic explanatory strategy of theories in the cognitive paradigm is by way of "extending the framework of FP to get an encompassing account of cognition in general."[4r] The way towards this is by extending the set of intentional constructs beyond those of beliefs, hopes etc., of which we may be conscious, to include unconscious, mentally represented rules. This strategy is replicated (or, arguably, was introduced) by Chomsky: in explanation of natural language the Chomskyan paradigm ascribes, not beliefs, but knowledge to individuals; tacit knowledge that is instantiated by a body of intentional constructs, the I-language's set of rules or principles and parameters. FP takes up an intentional stance and quantifies over psychologically real and causal propositional attitudes; Chomskyan linguistics takes up, what we might call, a knowledge stance and quantifies over mentally represented rules that are involved in the aetiology of linguistic behaviour.

FP's positing of beliefs and desires etc. as theoretical constructs in explanation of the behaviour of individuals constitutes a predictively accurate and remarkably successful "theory". The scare quotes because it is not a theory that has been arrived at through the normal route of conjecture and development. Rather, the consensus is that such a theory is part of our innate cognitive endowment as human beings.[5] However, it is one thing to propose that we are innately designed to spontaneously come up with such a theory and to point out that it works, it is another thing to propose that the theory is also true, works because it is true, and true because it corresponds to the psychological causal facts of the matter, i.e. there really are such things as beliefs and hopes that really are involved in the causation of behaviour.[6]

The former observation - that taking an intentional or knowledge stance works, because it is predictively successful - is separable from the latter claim, which claim is as to why taking that stance works. This is not to cast doubt on the objectivity of the patterns in behaviour which a theory correctly *characterizes*, but it is to distinguish this characterizing explanation from a causal account. What FP does is to take the terms/concepts by which we apprehend and successfully predict behaviour and preserve them in the causal account of that behaviour; what the Chomskyan model does is to take the calculus by which we gain epistemic access onto the linguistic, and by means of which we - as linguists - articulate theory, and also

preserve it in a mind-internal causal account. This is supposed to duplicate the innate theory through which we as acquirers/speakers gain knowledge of language:[7] it is what constitutes the language faculty.

The basic idea is not novel and goes back to the discussion of universals by the Greeks and, later, the scholastics:[8] in brief, to recognize some particular (e.g. a sentence token) as an instance of an x (a sentence type), one needs to always and already have the concept, i.e. universal, of an x (care of the generative capacity of the grammar). Another exemplar of this basic idea is Fodor's Language of Thought - to acquire a language one must already have a language - itself more or less a rehearsal of the view of St. Augustine as criticized by Wittgenstein at the opening of *Philosophical Investigations*.

What results, in general, are causal theories that mimic a certain conceptual necessity run off the conceptual apparatus that figures our apprehension of the behaviours. That the strategy works, e.g. in the linguistic case, by attributing knowledge of language to individuals, coincides with the account of how the mechanism works: there is an internally represented body of declarative linguistic knowledge, the cause is an abbreviated version of the effect. To reprise Searle:

> Epistemically, the existence of the patterns is taken as evidence for the existence of the representations. Causally, the existence of the representations is supposed to explain the existence of the patterns.[9r]

It is by no means impossible that such an account is correct, however, it is not an obviousness, nor is it obvious on the basis of the predictive success of the theory vis-á-vis speaker judgements, which is, in Chomsky's case at least, the argument of choice (and necessity, given the lack of anything else) to rebut scepticism. On the one hand, this explanatory strategy depends on the extent to which the hypothesis of mental representations inaccessible to consciousness can be made to hold water (as we discussed above (chapter IV, 2.2) Searle argues such constructs to be illegitimate), on the other hand:

> Those who think that it is obvious ... are confusing two different empirical claims. The first is that intentional [/knowledge] stance description yields an objective, real pattern in the world .... The second is that this real pattern is *produced* by another real pattern roughly isomorphic to it .... Doubting the existence of the

> second real pattern is not doubting the existence of the first.[10r]

Dennett suggests, as regards FP, that there are good, even if not overwhelming, reasons for believing in this second pattern. There are, however, also good reasons to doubt its existence. His position constitutes a third argument, to go with Quine's and Searle's in our previous chapter, that articulates a scepticism towards mental representations.

2.2 Dennett's instrumentalism.

As Dennett is at pains to point out, our FP constructs and explanations are not confined to our understanding of persons; indeed, taking up an "intentional stance"[11r] is endemic in our explanation of the behaviour of animate things in general (listen to the commentary on any natural history film), and frequently stretches to inanimate objects: a thermostat, for instance, turns up the heating because, we might say, it "believes" or "notices" the temperature is below sixty degrees. It is an intentional system by virtue of exhibiting an "aboutness" in regard to its environment. As regards a thermostat, or many machines, there is a good explanation as to why the intentional stance is successful: it has been designed by another intentional system so that it can be comprehended by individuals taking an intentional stance towards it; its intentional "mind" has been designed into it; it, as it were, borrows its "mind" from its designer.

Of course, we are not tempted to the view that a thermostat itself really has internal states of belief or represented rules which are causal of its behaviour, rather it has internal, electrical, states, that allow it to monitor (note how hard it is to avoid intentional talk) its environment and to act in response to it. In this regard our taking up an intentional stance is purely metaphorical, i.e. our "theory" with its intentional terms is acknowledged to be purely instrumental; a sort of short hand allowing us to avoid a complicated story of what is actually going on at the level of the electronics of the system. We feel no need to argue from the predictive success that taking an intentional stance affords to the causal reality, in the system itself, of the terms that we use. In other words, in respect of thermostats we can, if we have the patience and the knowledge, completely excise intentional terms from an explanation of their behaviour exchanging that level of description for one in terms of its electronics. This does not affect the predictive and objective success of our shorthand account: there are

objective patterns of behaviour that are captured by the characterization couched in intentional terms, but what we do not get is a causal account of the system in itself. Rather what we have is the system described in terms of our own interests, in terms of a certain conceptual apparatus, and preserving the terms of that FP apparatus in a metaphorical/instrumentalist account of the behaviour.

In the case of artefacts, the function computed is designed into the artefact, and it is from the perspective of the designer that some system can be appraised as either a better or worse instantiation of a level one function. The function, mediated through some engineering design, prefigures the artefact, it is sort-of-causally involved in the design of the artefact; it is the concept with which the engineer works. The reason why there are, for instance, mousetraps, is because someone thought of the concept (do we really want to get all Platonic and say that someone "discovered" it?) and designed an artefact with that concept/function in mind. Consequently, we understand the behaviour of the artefact in terms of the function. Mediated by way of the designer, the function is causal of the forms and patterns of the behaviour of the artefact when the behaviour of the artefact is considered in the light of that function. However, that the artefact's behaviour is an instance of the computation of that function is not something intrinsic to the artefact, rather it is something that is recognized only under interpretation. For example, that a mousetrap is instantiating the function of executing mice as opposed to wedging open a door is a case of someone having some function in mind.[12r]

Searle remarks that "the aim of natural science [recall, for Chomsky, linguistics is a natural science] is to discover and characterize features that are intrinsic to the natural world".[13r] If the level one functions of cognitive science are such intrinsic features (i.e. they correspond to real entities that are independent and waiting to be discovered, as opposed to being attributions out of the conceptual scheme of the attributer, e.g. the linguist), then what needs to be excised from an account of non-artefacts such as minds/brains is the third-person externality of the designing and interpreting mind which attributes the function, the "idea of the machine". The trick is to put the designing and interpreting, always and already idea in the mind of the individual whose behaviour is to be explained.

To take linguistics, Chomskyan style, as paradigmatic of cognitivist explanations, what replaces, by way of doing the job of, the external designer is the system of intentional rules which prefigure and "design" the patterns of the behaviours. In this way the "explanation" offered is not dissimilar to pre-Darwinian

creationist accounts of natural phenomena: to explain why something happens to be as it is one invokes the prefiguring and designing mind of the Deity; the phenomena of the world are simply material instantiations of His prefiguring mind.[14] Dennett supplies us with an example. The wasp species Sphex is an instance of a lower organism that evidences "intelligent" behaviour. Before laying its eggs, the female of the species builds a burrow, stings a cricket to paralyse it, drags the cricket into the burrow, lays her eggs next to it (it will be her progeny's larder), closes the burrow and flies away.[15r] On a creationist's story:

> The behavior of the wasp is explained ... by postulating the existence of an intelligent mind, a god ... who thought about and *designed* the behavior of the wasp, based on his knowledge of the environment of the insect, and who subsequently *created* the behaviour he designed, who gave it material shape in the wasp.[16r]

In the case of human intelligent behaviours, the cognitivist paradigm makes the move of transferring wholesale such "knowledge" into the mind/brain of the individual by way of a set of mentally represented rules, the competence grammar, which designs and is involved in the "creation" of the behaviours. On the creationist model, the intelligence of the wasp is offered as evidence for the existence of a mindful creator, likewise, on the cognitivist model, the existence of the intelligent behaviour is taken as evidence for the designing and prefiguring "knowledge" by way of a representation of the function that is computed in the behaviour. (Compare our previous quotations from Searle and Dennett above, (pg. 192)).

But what is also happening in this style of explanation is that the conceptual terms under which the behaviour is apprehended and understood are preserved in the cause, (as in the creationist's account) and are preserved in the manner of abbreviated or condensed always and already versions, as the prefiguring idea of the patterns of the behaviours. What is being achieved here is that the identification of level one functions, as a proper level of description of mind/brain internal states, is no more than a case of projecting the terms of the conceptual apparatus with which we theoretically apprehend and interpret forms of overt and particular behaviour back onto the mind, in the form of mental representations, and so preserving the terms of the conceptual apparatus that provides us with epistemic access to the phenomenon in a causal account: a form of putatively empirical enquiry driven by a species of conceptual necessity. What we get is something which is akin to what we dubbed a

skeuomorph effect: a case of taking the terms, (in effect, a particular set of universals,) under which a phenomenon is represented/apprehended and transposing them into the phenomenon and construing them as intrinsic to that phenomenon at the phenomenon's causal level. The strategy to "explain" the observed structures and patterns of human cognitive behaviours is to posit those structures and patterns to be always and already more or less explicit in the cause as something that begins to look rather like a sort of conceptual homunculus. It is a case of explaining the universals that are instanced in particular behaviours, at least as those particulars are apprehended by, for example, linguists, by positing the set of universals, i.e. grammars as, (again some old-fashioned vocabulary) their first cause in the mind of, in the linguistic case, the speaker of a language.

> In certain respects, the current scientific state of a general theory of representation is analogous to the science of embryology in the nineteenth century. The development of highly structured, complex, fully formed organisms from eggs and sperm is a profoundly amazing thing. Faced with this mystery, some scientists concluded that the only way to explain the emergence of a fully structured organism at birth was to join the ancients in assuming that the structure was already there. Hence the homuncular theory of reproduction, which claimed that a miniature but complete human already exists in the sperm ....
>
> We now know that there *is* structure in the sperm (and the egg) - not in the form of a miniature, fully structured organism, but mainly in the form of DNA - a molecule that looks not at *all* like a fully formed human. Thus, the structure of the cause does not resemble the structure of the effect.[17]

However, the rejoinder is to point to the explanatory success of cognitivism's strategy of postulating level one functions as mentally represented rules, i.e. in taking a knowledge stance, and of the parallel strategy in FP of postulating beliefs and desires *et al.*, in taking an intentional stance. The point is simply that in both cases the strategy works and such mental representations appear indispensable to an explanation. The question is why it works. Either such representations are really used (i.e. they are involved in the aetiology of the behaviour) or it is no more than "as if" they are used (compare our quotation from Berwick, (pg. 142)).

The non-causally explanatory option is canvassed by Dennett. Dennett's position is open to a degree of interpretation.[18] However, for our purposes the crux is that in viewing some system as intentional (or as a knowledge system), one attributes to that system a range of beliefs (or knowledge states). But, (compare our thermostat) any single description from either an intentional/knowledge stance is compatible with indefinitely many system-internal states of affairs (there are many different ways to design and implement a thermostat): what is criterial is the external behaviour. But what this suggests is that:

> the beliefs and other intentions of an intentional system [or knowledge states] need not be *represented* 'within' the system in any way for us to get a purchase on predicting its behavior by ascribing such intentions [or knowledge states] to it.[19r]

Rather, these postulated states, taken as constituting part of a *causal* account of the behaviour, warrant, as we have been suggesting, no more than an instrumentalist construal. Indeed, Dennett suggests that they require to be interpreted as "idealized fictions in an action-predicting, action explaining calculus."[20r] Such states exist not as *illata,* as entities that exist independently in the world, but are "*abstracta* - calculation bound entities or logical constructs"[21r] projected from the conceptual apparatus by which we represent the world. In this Dennett is close to Searle's contention that "the so-called 'functional level' is not a separate level at all, but simply one of the causal levels *described in terms of our interests*". The I-language does not correspond to an *illatum*, but is a fictional entity projected from the conceptual apparatus that is our means to represent, and this *representans* is mistakenly posited as *representatum*: a case of a skeuomorph effect. It might also be noted that the fictionality of the grammar *qua* internal system is entirely consonant with the empirical strandedness of psychologistic linguistic theories: fictions do not leave traces of themselves.

On the other hand, there is, Dennett insists,[22r] something perfectly objective about the descriptions of the behaviours that are available by taking an intentional/knowledge stance and which descriptions can only be captured in this way. But whatever this something perfectly objective is, such as it constitutes certain facts of the matter, they need not be system-internal, intrinsic facts, rather only facts as appear under attribution. Moreover, there are an indefinite number of such sets of facts given that for any level one function, e.g. a grammar, there are an indefinite number of extensional equivalents of that function/grammar. Insofar as they are

predictively successful, grammars can not be licensed to be taken as true of some mind/brain internal states, but to be true of the external patterns of linguistic behaviours. But, if this is the case, then we have to drop a psychologist ontology, correlatively also drop any truth-issues in respect of a choice between grammars that achieve coverage, and we appear to be in a position that recapitulates a Popperian third world ontology (or a Platonism), for whatever it is that are the conditions of identity for patterns of behaviour as being instances of a natural language cannot be system-internal/psychological.

While such instrumentalist contentions vis-á-vis a causal explanation might seem entirely reasonable in respect of artefacts like thermostats, we are, perhaps, less taken by the prospect in regard to our own behaviour, for does it not rather offend our dignity, not to mention our understanding of ourselves? Moreover, it is at least arguable that in a certain sense the apprehension of the behaviour of others in FP terms is, indeed, ineliminable. If, as the currently fashionable story goes, and however it is to be explained, we are so constructed so as to develop a folk psychological theory which predisposes us to cast the behaviour of other individuals in its terms, then it is conceivable that this form of apprehension is in the manner of an indefeasible reflex. Insofar as our conscious apprehension of other persons involves FP terms, then these terms might well be ineliminable: maybe we cannot help but think of people's behaviour in the FP framework. If there really is an innate theory of mind, then it is part of what constitutes our very rationality to understand individuals' behaviour in terms of FP's constructs. To this extent, as Putnam argues, FP would not, in a certain sense, be corrigible at all.[23r] If it turned out that a true account of behaviour eliminated, at all levels of description, the constructs of FP, then, feasibly, we would still, as a reflex, apprehend and understand behaviour in terms of beliefs and desires. Similarly, for instance, the sound of a flute is always the sound of a flute and not what the physicists tell us it *really* is, i.e. a sinusoidal wave train. However, we simply do not naturally apprehend the sound of a flute in the physicist's terms. In hearing the flute as a flute, but knowing some physics, we are in a position akin to looking at an optical illusion, knowing it to be so, but still, and simultaneously, the illusion persists.[24]

2.3 Corollaries.

The conclusion these remarks tend toward is that FP is not a necessarily true theory of the aetiology of behaviour (not unless we are tempted to the view that evolution, which is presumably responsible for FP, is geared towards selecting for organisms that apprehend the world as it really is, as opposed to selecting for organisms whose apprehension of the world confers an advantage on the organism[25]). We seem to have to confer no more than an instrumental status on the intentional constructs of FP and this suggests that they are viable candidates for elimination from a correct account. The point of our addressing this question is that if the FP strategy of attributing such constructs to persons can be brought into question, then, given the parallels between taking an intentional stance and taking the knowledge stance, this will also bring the supposed ineliminability of the MRG hypothesis into question. It is to be noticed that one of the factors that argues for resistance to elimination of the former, i.e. the availability to consciousness of our FP intentional constructs, is, by definition, not available in support of the hypothesis of tacit/unconscious intentional constructs. In other words, while one might resist the elimination of FP constructs by appeal to the fact that such terms are those which populate our conscious awareness and are which we think with, how much less unappealing and less counter-intuitive is the elimination of constructs/terms of which we are unconscious.[26]

Our general question now gets couched in terms of the prospects for elimination of FP (and so, on the parallel we are drawing, of MRGs). For a précis of the various positions on the question I can do no better than quote Churchland:

> The identity theorist optimistically expects that [FP] will be smoothly *reduced* by completed neuroscience, and its ontology preserved by dint of transtheoretic identities. The dualist expects that it will prove *ir*reducible to completed neuroscience, by dint of being a nonredundant description of an autonomous, nonphysical domain of natural phenomena. The functionalist also expects that it will prove irreducible, but on the quite different grounds that the internal economy characterized by [FP] is not, in the last analysis, a law-governed economy of natural states, but an abstract organization of functional states, an organization instantiable in a variety of quite different material substrates. It is therefore irreducible to the principles peculiar to any of them.

> Finally, the eliminative materialist is also pessimistic about the prospects for reduction, but his reason is that [FP] is a radically inadequate account of our internal activities, too confused and too defective to win survival through intertheoretic reduction. On his view it will simply be displaced by a better theory of those activities.[27r]

The crux of the position that we are arguing for is that, firstly, as we have demonstrated, despite Chomsky opining in the direction of some form of promissory identity between the constructs of a postulated I-language and "structures of the brain", with a correlative reduction of the linguistic to biology, this position lacks any substantive evidence from neurophysiology, nor, indeed, from any purportedly relevant adjacent domain. The unsubstantiated nature of his position and its inexplicitness is negotiated by the invocation of the "level of abstraction [of the investigation] from mechanisms" and the "complex question" as regards "what is meant by the notion of 'corresponds' in the case of [this] abstract study".[28r] The upshot of this level of abstraction with its correlative empirical insulation is, in effect, a realism and a materialism that is negotiated by way of the enthymemes of an implicit functionalism.

3.0 Functionalism.

3.1 Origins.

Functionalism developed in response to problems that are attendant on mind/brain identity theories.[29r] The nub of the matter is that if one identifies mental types, such as pain, with types of neurophysiological state (type-type identity), such as having certain specified brain fibres firing, the result is a counter-intuitive inability to predicate of some organism that it is in pain unless it also has just the same type of brain fibres firing. The consequence is a type of species chauvinism where an organism is precluded from, to continue in our example, being in pain unless it shares the same neurophysiology as humans. The solution is to propose that what is crucial is not what type of neurophysiological machinery is involved (imagine, in the human case, that some future medical advance allows pieces of damaged brain to be replaced with bits of hi-tech circuit board), rather, what is important, what is the criterion of

identity, is what that piece is doing in respect of the cognitive functioning of the organism as a whole. Types of mental state get to be identified with an abstract functional role, not with bits of neural wetware. This position does not require the relinquishing of a thorough-going materialism: individual instances ("tokens") of pain may indeed be identified with particular neurophysiological events in an individual, (upon which neurophysiological facts they are supervenient,) but pain as an abstract type of mental state cannot be so identified.

The result is the mobilization (compare Marr) of distinct levels of description:

> but [the functionalist] applies them all to the same fundamental reality. A physical state-token in someone's brain at a particular time has a neurophysiological description, but may also have a functional description .... And so there is after all a sense in which "the mental" is distinct from "the physical": though there are no nonphysical substances or stuffs, and every mental token is itself entirely physical, mental characterization is not physical characterization, and the property of being a pain is not simply the property of being such-and-such a neural firing.[30r]

Moreover, the abstract types of mental states, correlating with level one functions, are ineliminable, by reduction, from any causal account of cognitive functioning, because, given the multiple physical instantiability, any description at the level of the neurophysical would not be able to capture what is common to instances/tokens of mental types. In short, "a functional characterization of our internal states is here to stay."[31r] And also here to stay is the level of abstraction that Chomsky cites as a methodological and provisional requirement, only it is not provisional because not eliminable.

3.2 Multiple instantiability.

A consequence of the claim as to the ineliminability of a functional characterization is the strandedness of level one theories from (dis-)confirmation by neurological evidence. If the same function may be realized in a variety of physical systems, where that variety includes systems, many of which are constituted of substances to which different physical laws apply, then findings about the physical

system *qua* physical system do not constitute any sort of evidence as to the correctness or identification of any level one theory: there can be no reduction of the level one theory in respect of any laws that apply at the physical level. In a similar regard, Fodor points out,[32r] invoking the standard mind-as-computer metaphor, that it is quite possible for two computers to be in precisely the same physical/electronic state but each computing a different function; one, for instance, calculating the gross national product of Monaco, the other the batting averages of the New York Yankees. In other words, one cannot tell from inspecting the hardware, nor the algorithm, just what is going on at the level (level one) of what is being computed. Conversely:

> It seems possible for two individuals to be in the same mental state even though they're in different computational states. Two computers can multiply 35 and 44 [or the GNP of Monaco] in very different ways. By analogy, two sentient beings might possess some of the same psychological [for our purposes, linguistic] properties even though their psychologies involve very different programs [i.e. grammars]. The computational model of the mind thereby suggests, surprisingly, that mental [/linguistic] states are *not* to be identified with computational states [/a specification of an I-language].[33r]

And, of course, neither are they to be identified with neurological/biological/physical states. The question arises as to precisely what they are to be identified with; what, precisely, (of what ontological status) are mental states/level one functions if the specification of the internal state of a machine/individual, even to the level of the economy of information of any putative internal representations of some program, is redundant in their identification? It seems there are no truth-issues in respect of a choice between extensional equivalents, it seems there is no cogency, because no identifiable content, to the notion of an I-language (compare Quine). Correlatively, we seem to need an E-language approach and an ontological status which is indistinguishable from either Carr's/Popper's autonomism or a Platonism à la Katz, and this because the identity of any level one function is ineliminably distinct (and its properties underivable) from any internal states of a machine/organism. What this appears to suggest is that a linguistics that posits a level one function as its central explanatory construct is orthogonal to, or at least distinct from, a linguistics conducted from the perspective of "individual psychology". I suppose one can get to call the

former construct "psychological", but it is psychological in a wholly object-of-a-propositional-attitude sense. Katz makes the same point.

He asks us to imagine an English speaking Martian who, presumably, has a different neurology and who also employs a different but extensionally equivalent program to the one supposedly internally represented by human English language speakers. Neither internal fact has any relevance to the external, mind/brain independent fact that he/she/it is still speaking English. The existence of English is independent (ontologically) of any particular mode of its instantiation. The upshot is that the identification of I-language with Marr's and functionalism's level one results in anomaly for an explicitly reductionist psychological linguistics by way of undermining the possibility of a reduction to the properties of the instantiating hardware. But also, it appears to undermine any psychologist (human-mind internal) ontological claim and correlatively the cogency of an I-language, i.e. a determinate set of mind-internal representations, as the proper object of study. In short, it appears not the case, contra Chomsky, that "language has no objective existence apart from its mental representation,"[34r] for, indeed, it *does* have such an objective existence. Katz puts the point succinctly:

> We cannot abstract away from [mental or neural structures] without abstracting away from the psychological medium in which competencies are realised .... Such abstraction would collapse conceptualism [i.e. mentalism] into Platonism [or, at least an autonomism].[35r]

The point is not, of course, restricted to linguistic matters: level one functions in general appear to require to be granted a status as being autonomous of any physical and representational state of affairs.

One corollary of multiple instantiability is that it undermines any rationale for choosing between extensionally equivalent level one theories in respect of any mind/brain internal facts of the matter, that is, in respect of any facts of the matter as regards the manner of instantiation of the level one function, because any such facts are beside the point: east is indeed east and west is indeed west, the level one function is always and already, and is so independently of the internal states of any individual minds/brains/computers, indeed, to take this independence to its Platonic conclusion, seemingly independently of the existence of any such entities. It seems, in fact, that what we are led towards concluding is that level one functions look remarkably like

reasons for having to propose an ontological category along the lines of Popper's world three or Katz' Platonism.

A further corollary is that the putatively ineliminable level of description of a level one function, in being raised above any particular instantiation, appears to remove a functional characterization from empirical criticism, leaving the level one theory empirically stranded. What results is "a faintly stipulative character," one might say, a certain conceptual necessity: it is "as if the onus were on the empirical systems to instantiate faithfully the organization that [level one functions] specify".[36] The sense is that of the specified function prefiguring what must be the case in the physical system. What we get is the function as a sort of picture, symbol or idea of an ideal machine, of, in Wittgenstein's phrase, "the machine-as-symbol" which, as it were, contains the movements of any actual machine in a manner far more determinately than any imperfect actualization and which idea is, indeed, independent of any actualization.[37r] The impression of the incorrigibility of the specified function by any merely empirical facts is, Churchland observes,

> enhanced by the standard examples used to illustrate the claims of functionalism - mousetraps, valve-lifters, arithmetical calculators, computers, robots and the like. These are artifacts, constructed to fill a preconceived bill. In such cases, a failure of fit between the physical system and the relevant functional characterization impugns only the former and not the latter.[38r]

An alternative way to express the point, and one which is more clearly of linguistic relevance, is to remark on the putatively principled discrepancy we remarked upon between competence, the level one characterization, and the performance of any particular system. The fact of the performance mechanisms in some way falling short or diverging from what is licensed by the competence system does not bring into question the correctness of the specification of the competence system because that system is autonomous of, and unfalsifiable by, any evidence regarding algorithmic processing. It seems the function is known in advance of, and irrespective of, any empirical evidence.[39r]

The conclusion that these considerations from the multiple instantiability of level one functions tends towards is, again, that of their autonomous, abstract ontological status;[40] if they are not to be identified with any particular state of a

physical system, and not corrigible in respect of any particular system, then they seem to have some sort of existence, or subsistence, that is neither spatial nor temporal, they appear:

> not [to be] phenomena located in space-time and we lack any evidence at all for their existence. Nevertheless they have to exist [spatio-temporarily], because they are part of the [causal explanatory] paradigm of Cognitive science.[41r]

And, if they have no such spatio-temporality, and they are to be credited with existence, then either they are the sorts of things that inhabit Popper's world three or, to taste, Platonic entities.

It is at this point that the functionalist's drift to autonomism, along with its unpalatability to materialist sentiments, is attempted to be arrested by the invocation of the notion of supervenience or of a promissory neuro-science - the enthymemes of Fodor's remarks. It is not the case, so the story goes, that level one functions exist in the ether, they only exist because they are instantiated in physical systems and depend on such physical systems for their existence: no physical system, no level one function. This is not, however, to impugn the truth and ineliminability of generalizations made at the level of level one functions.

This seems to be Fodor's position. As regards the question of the ontological status of the mental, he holds it "very likely" that, without espousing a reduction of mental types to physical types, "mental events have true descriptions in the vocabulary of an ideally completed physiology."[42r] What we get, by way of this act of faith, is a discharging of the need for abstract ontological categories; we have the advantage of maintaining a materialism without being committed to a thorough-going reductionism: significant and true generalizations can be made without reference to the physical properties of the instantiating system. But, this is just the promissory again, and what we also have is a present inability "to distinguish in practice between ... [Fodor's] *de facto* anti-reductionism and ... a radically anti-reductionist" autonomism.[43r] An autonomism might be embarrassing to our materialist prejudices, but an autonomism is what we get. The only other option appears to be to question the (nature of the) existence of the always and already level one function.

4.0 Eliminating level one.

These remarks are intended to have been taken as casting doubt on the legitimacy of cognitivism's and Chomskyan linguistics' explanatory strategy in taking a knowledge stance; a case of raising the antecedent improbabilities of an explanation of behaviour in terms of the mental representation of level one functions. Raising those improbabilities is to undermine acceptance of theories based on their explanatory success - some inference to the best explanation: they might explain, provide a story, but the constructs through which the story is achieved are problematic. It might be noted, however, that none of the above considerations are sufficient to exclude the possibility that indeed there are internal states that correspond to the construct's of FP's intentional stance or the constructs of Chomsky's parallel knowledge stance. Furthermore, it is arguable (compare Stabler's remarks above, (pg. 154)) that such doubts as have been raised leave the practice of linguistics in the Chomskyan paradigm unaltered, for whether or not we take the causal explanation realistically we still have an agreed and perfectly objective object of study: the level one function. However, if it can be further substantiated that this object of study is indeed incompatible with any, even a promissory, mind-internal psychology (we take this up in the next chapter) and so leaving us not only with a deferred explanation but the impossibility of an explanation from the perspective of "individual psychology", then we might be encouraged to reassess the status/reality of level one functions.

Just such a reassessment is suggested by Searle where he takes up the challenge of showing how such theoretical constructs as systems of mentally represented level one functions might be excised from explanation.

Searle's worries about the explanatory strategy of cognitivism amount to the observation that, firstly, where we properly understand certain systems the postulation of a functional level explanation is no part of their explanation, and, secondly, that where we do have computational simulations of systems those systems do not themselves make causal use of a represented level one function.

For example, a word processing program in a computer is causal of the simulation of the output of a good, old-fashioned mechanical typewriter. Certainly, this program is causally explanatory of the workings of the computer, but it is not in the least bit involved in the understanding of how a typewriter works:

> As simulations go, the word processing program simulates a typewriter better than any AI program I know of simulates the brain. But no sane person thinks: "At long last we understand how typewriters work, they are implementations of word processing programs."[44r]

To this it might be objected that what is crucial to the working of a typewriter, what in fact provides the program, is the mind/brain of its operator.

However, consider what happens when sitting in a moving car and focussing on the road ahead. Were a video camera to be set up to film as you travelled along, the resulting images would be shaky and jerky. This is not replicated in your own visual experience; instead you enjoy a more or less stable and smooth view of the road. One might explain this phenomenon in terms of a mentally represented level one function amounting to an unconscious and always and already rule to move the eyeballs in your head in such a way as to maintain focus on some intended object and so compensate for the involuntary head movements induced by the bumpy road surface. Such a rule would explain complex, flexible, goal-directed behaviour involving information processing: the rule takes in information (from head movements) as input and gives as output the desired behaviour. It seems we have a canonical cognitivist explanation in response to just the sort of problem which cognitivism was set up to handle. Moreover, we have a predictively accurate explanation once the function (or any extensional equivalent) is correctly specified. The trouble is that what *actually* happens is rather different:

> What actually happens is that fluid movements in the semicircular canals of the inner ear trigger a sequence of neuron firings that enter the brain over the eighth cranial nerve. These signals follow two parallel pathways, one of which can "learn" and one of which cannot. The pathways are in the brain stem and cerebellum and they transform the initial input signals to provide motor output "commands", via motorneurons that connect to the eye muscles and cause eyeball movements. The whole system contains feedback mechanisms for error correction. It is called the vestibular ocular reflex (VOR). The actual hardware mechanism of the VOR [involves a system of mentally represented rules] no more than [does] the movement of [a] plant's leaves due to the secretion of auxin. The appearance that there is an unconscious rule

> being followed ... is an optical illusion. All the intentional ascriptions are *as-if*.[45]

The question this explanation raises (and, for our present purposes it is not crucial that it is a neurological explanation) is that of how one is to consider the status of the still predictively accurate specification of the level one function that characterizes the VOR. Certainly, we come to see it similarly to the way we recognize intentional ascriptions to thermostats, as metaphorical/instrumental merely, but what, ontologically, is that level one function? It would appear to be no more than an abstract characterization of a set of overt behaviours: an idea of the machine, but the proper explanation is to be found not in understanding the idea but in understanding the machine itself. It is the machine which is prior and not some always and already level one abstract object: insofar as an enquiry posits such level one abstractions they are abstractions only in the sense of being derived by generalization, idealization or classification over the real data, namely the behaviours of the (neuro)-physical machine; what is abstract in the enquiry is not of the object of enquiry, but merely of-and-for-the-scientist, having a merely instrumental utility in the scientist's efforts to come to grips with the real object of study.[46r]

In effect, at least interpretably, Searle's position amounts to (as we will see in the next chapter where we take up the issues raised here) a Wittgensteinian exhortation to look at the machine itself as opposed to the function that is the idea of the machine. To put this in old-fashioned terminology, and to return us to our comments at the opening of this chapter, what is being canvassed amounts to a distrust of universals (in the guise of mentally represented rules of which the intentional content is tacit) as causal explanations for behaviour, where those universals are postulated as a means to preserve, in some putative cause, a realist construal of the terms/conceptualization by which a theory attains epistemic access onto a phenomenon.

Footnotes.

[1] The answer to the question is, of course, no. The attempt to negotiate the abstraction of the mental within a materialism is, however, what underpins the functionalist enterprise.

[2] This observation goes back, famously, to Brentano (F. Brentano, 1874) and retains a considerable currency, although the present consensus is that the mental is not adequately defined in this way (for example, pains are mental if anything is, and pains do not appear to have any directedness or "aboutness").

[3] For an overview see A. Whiten, (ed.), (1991).

[4] P. Churchland and T. Sejnowski, (1990, pg. 232).

[5] The argument for this view very accurately recapitulates, *mutatis mutandis*, Chomsky's for an innate language faculty:

> By the time children reach the age of three years old they attribute mental states to other people when attempting to explain their actions. In particular, they understand that other people have beliefs and desires and that these play a causal role in behaviour. ... The basic concepts of belief and desire that children use, whatever their cultural background, could not be constructed from the evidence available to them during the earliest stages of their development. Consequently these concepts appear to derive from an innate psychological structure - a content-rich mental module which creates mandatory interpretations of human behaviour in mentalistic terms. (S. Mithen, (1996, pg. 51)).

Other candidates for innate "theories" or modules are, along with language, an intuitive biology and an intuitive physics. Somewhat more profligate proposals (what Fodor calls "modularity gone mad" - see chapter I, footnote 83) have also been suggested (see Sperber (1994), Tooby and Cosmides (1992) and Barkow *et al.* (1992)).

[6] If you consider the raising of doubt here as a case of scepticism taken to absurd lengths, consider the following report of neurological experimental findings and try to reconcile it with a FP account which would, presumably, have the conscious, mental decision as having a causal role:

> if somebody is asked to move a finger voluntarily, and to say when he *first* decides to make the motion, then waves of brain activity in the relevant motor area start up a second or two *before* he says that his conscious mind "makes the decision." (Cohen and Stewart, (1994, pg. 176).)

[7] Recall Chomsky's remarks as to the "systematic ambiguity" of the term "grammar": "to refer, first, to the native speaker's internally represented 'theory of his language' and, second, to the linguist's theory of this" (1965, pg. 25).

[8] The standard options on universals include: a) Platonism, they exist *ante rem* (compare Katz); b) the Aristotelian view that universals are *in re*, and not independent of particulars (compare Fodor's view below); c) nominalism, the view of certain of the scholastics, that universals are no more than *flatus vocis* and eliminable (compare the Churchlands, and Searle).

Russell (1979, pg. 430) remarks, writing in the forties, that "modern discussions of the problem of universals have not got much further [than those of the scholastics]" and arguably this is still the case.

[9] J. Searle, (1992, pg. 241).

[10] D. Dennett, (1990, pg. 164).

[11] The phrase is Dennett's, (1987 and *passim*).

[12] See R. Cummins, (1989) and J. Searle, (1992, pgs. 205 - 212).

[13] *Ibid.* pg. 212.

[14] Again there are interesting parallels with the scholastics' treatment of universals. For instance, Abelard, Occam and Aquinas, while all of distinctly nominalist tendencies, felt the need to suggest that universals are *ante rem*, only in the sense that they are concepts in the mind of God, and this has to be the case so as to explain creation: such ideas "had to be in the mind of God before he could create" (B. Russell, 1979, pg. 464).

[15] D. Dennett, (1978). Cited in W. Meyer Viol (ms).

[16] W. Meyer Viol (ms).

[17] P. Churchland and T. Sejnowski, (1990, pg. 234). Compare with the notion of epiphenomena (chapter I, footnote 78.).

[18] D. Dennett, (1978, 1981 and 1990). The interpretive issue is one of the extent to which his "mild realism" constitutes, or does not, a thorough-going instrumentalism in respect of FP constructs. For conflicting reports see, for example, the entry in T. Honderich (1995) and T. Horgan and J. Woodward (1985). Stich's (1981) paper, in noting that Dennett has "a disconcerting penchant for working both sides of the street", investigates them both.

[19] D. Dennett, (1978, pg. 277).

[20] *Ibid*, (pg. 30).

[21] D. Dennett, (1981, pg. 13).

[22] D. Dennett, (1990, pg. 158 - 159).

[23] H. Putnam, (1964, pgs. 681ff.) Also see N. Chomsky, (1980, pg. 7).

[24] This observation is akin to Husserl's identification of a *Lebenswelt* ('life-world') (E. Husserl, 1970): "the inter-subjective world of our natural, pre-theoretical experience and activity ... [which] *essentially* persists even after the development of the theoretical 'spirit'. Even the physicist thinks of the sun as rising and setting, and as marking the phases of his practical life" (from the entry on Husserl in T. Honderich, 1995, pg. 384).

Arguably, a similar distinction between the scientific 'theoretical attitude' - exemplified for Husserl by Galileo - and that of pre-theoretical, everyday experience may be discerned in the later Wittgenstein's anti-scientism (see Chihara and Fodor, 1967).

[25] If we concede that evolution selects for innate theories, then, given our understanding of the nature of evolution, best theories, i.e. the ones selected for, are likely to conform to something much closer to a pragmatist's notion of truth, rather than some sort of correspondence theory. While it might be advantageous for an organism to get it right, more appropriate, on some evolutionary story, will be a theory's success in action in respect of the interests of the organism and this may, or may not, involve getting it right, in the sense of corresponding to how things really are. For example, imagine there were some evolutionary advantage involved in being able to predict the motion of the planets and there were to evolve an innate theory. There would be no reason for evolution to favour a heliocentric (and correct) theory over a geocentric/Ptolemaic (and wrong) one as long as they both do the same job in respect of whatever the advantage is that is accrued by having such an innate theory. In short, just because a theory, eg. FP, is a product of evolution does not confer on it the status of a true theory.

[26] This point approximately rehearses Searle's distinction between "intrinsic" and "as if" intentionality and his argument for the "Connection Principle" (see chapter IV, 2.2). This, it will be recalled, is to the effect that the only cogent sense that can be made of intentional constructs is if they are candidates for being contents of consciousness; where states are not candidates for being contents of consciousness, the intentionality is no more than "as if", no more than metaphor, and for which the proper description is neurophysiological. (Searle, 1992).

[27] P. Churchland, (1990, pgs. 209 - 210)

[28] The former quotation is from Chomsky, (1986, pg. 23) and the latter Chomsky, (1980, pg. 220).

[29] The seminal papers are H. Putnam (1960 and 1967) and J. A. Fodor (1968). It is worth noting that Putnam has repudiated the position he was instrumental in founding.

[30] W. Lycan, (1990, pg. 8).

[31] P. Churchland, (1990, pg. 213).

[32] J. A. Fodor, (1980).

[33] E. Sober, (1990, pgs. 98 - 99).

[34] N. Chomsky, (1972, pg. 169).

[35] J. Katz, (1981, pg. 91).

[36] P. Churchland, (1990, pg. 213). Compare Chomsky's emphasis (e.g. 1986, pg. 39 and our remarks above, (pg. 157)) on the directionality between the linguistic and adjacent domains being from the former to the latter: it is linguistics that constitutes a problem for the brain sciences rather than the other way about.

[37] L. Wittgenstein, (1958, sections 193 - 194).

[38] P. Churchland, *ibid.*

[39] See for example, N. Chomsky, (1991, pg. 19).

[40] It might be noted that the issue of multiple instantiability is related to the issue of the need, if constructs of a grammar are to be taken as real, for there to be means to effect a decision between extensionally equivalent grammars. To rehearse a Quinean view, it appears that the only facts of the matter are in the external behaviours.

[41] W. Meyer Viol, (ms).

[42] J. A. Fodor, (1975, pg. 9).

[43] P. Carr, (1990, pg. 91).

[44] J. Searle, (1992, pg. 218).

[45] *Ibid*, (pg. 236). This may also count as another lesson to the purpose of illustrating that "heuristic fertility" is not sufficient warrant for a realism.

[46] This sentence is an adulteration of remarks by Burton-Roberts, (1994, pg. 189). Compare pg. 121.

CHAPTER VI.

Ghosts, Machines and a Linguistic Constructivism.

Words move, music moves
Only in time...
Or say that the end precedes the beginning,
And the end and the beginning were always there
Before the beginning and after the end.
And all is always now.
(T. S. Eliot, *Burnt Norton.*)

We are talking about the spatial and temporal phenomenon of language, not about some non-spatial, non-temporal phantasm. [Only it is possible to be interested in a phenomenon in a variety of ways]. (L. Wittgenstein[1r].)

In itself language is ... an activity. (W. von Humboldt.[2r])

An utterance ... is a structure in time, ... in which the wholes and parts are events. ... Time's arrow applies to human behaviour as it does to the rest of the universe. (C. Hockett.[3r])

1.0 Introduction.

What emerges from our discussion of Searle's and Dennett's perspectives on cognitivism is a view of level one functions as corresponding to a statement of the laws of cognitive behaviours akin to a constitutive Keplerian statement of the laws of planetary motion. Such laws are not to be quantified over as causally explanatory constructs, but are abstract constructs that are no more (and no less) than a conceptual and objective instrument by which to predict and understand possible realized behaviours.

On the Chomskyan account, the laws are to be quantified over, they are supposed to correspond to determinate neural structures and are posited as part of the causal explanation of linguistic behaviours. However, those behaviours are not themselves the primary linguistic realities and objects of study: rather the object of study is the I-language, the set of rules and parameters, which constitutes the always

and already of the linguistic; it is, as a level one function, logically distinct from, prior to and in excess of any performance: it is, as it were, the ghost in the machine and which ghost is the understanding (in both senses of the ambiguity) of the machine.

Platonism/autonomism, on the other hand, directly quantifies over the set of always and already abstract sentence types that constitute a language, again an object that is transcendent of actuality and performance: the primary commitment is not, or cannot be to a particular formulation of the laws/grammar, but to the mind-independent objects that those laws are in answer to through their generative capacity.

Our purpose in the preceding chapter was to question the antecedent probability of the MRG hypothesis as a causal explanation. To the extent that we have raised the improbability of this explanatory move, then this is the extent to which a realism regarding the object of study of linguistic theory needs recourse to a Platonism/autonomism and, correspondingly, to the positing of the primary - as opposed to derivative - object of study as the set of sentences of a language. What we have suggested is that Chomsky's generativist paradigm, in its realism, fails to provide content to its psychologism and collapses ontologically into, because it is empirically indistinguishable from, a Platonism/autonomism: a ghost without a machine. The generativist object of study is orthogonal to, is resistant to incorporation into, a perspective from individual psychology. It requires, and is routinely conceptualized as an always and already Platonic object.

However, what we have also suggested is that such an ontology is not a solution, but is rather the problem and a problem which is sufficient motive to bring us to question the reality of the object of study instituted by the generativist paradigm. This is because, once the psychological locus is removed, then we neither have an explanation of the human language faculty - neither how it came to be (there can be, as indeed there is not, an evolutionary account), nor an account that can avoid taking refuge in mysteries as to how the individual gets into any relation (causal or epistemic) with linguistic objects. It is precisely an answer to this latter question which it is the ostensible purpose of Chomsky's psychologism to provide and which, we have argued, has failed to be achieved: the object of study, the competence grammar of the generativist paradigm has failed to be, and resists being, assimilated into a psychology; there is simply no ascertainable psychological content for an I-language.

Our central conclusion is that from the perspective of psychology a competence grammar, *qua* causal explanatory construct, cannot be afforded, because there is no evidence for, any more than an instrumentalist construal. The purpose of the present chapter is to, firstly, show that not only is there no such evidence, but that

the positing of a psychological ontology for a competence grammar induces tensions that approach incoherence (an incoherence that is tidied up and disguised by liberal application of Occam's broom). Secondly, and running in tandem, we will argue for the irreality and instrumentality of the postulated object of study, i.e. the object, the ghost, which remains once one has undermined the psychological locus of the I-language: an always and already set of sentences. What generativism has answered to is a second order idea of the linguistic, an idea to which there is nothing that answers in reality. It is an idea that takes the place of the actual and which, where the actual does not answer to it, then this is not to impugn the idea but is to impugn the actual as an aberration from this transcendent reality. There is, at least, the promise of a certain consonance in this diagnosis: it is likely that you need a causal/explanatory fiction to answer to a fiction. Finally, we will take the observation of the radically context-dependent nature of linguistic objects to argue for the need for a linguistics that is concerned with the linguistic as, at root, a constructive procedure: the linguistic is to be found always and only in such psychological constructions and events; what we are to be concerned with is ex-(or)-cising the ghost, Wittgenstein's "phantasm". A language is not so much something we know, something in which (we have the illusion that) "the end precedes the beginning", rather it is something that we do: it is, as von Humboldt observed, firstly and foremost an activity and as such it is required to be understood.

2.0 Antimonies of Chomsky's Psychologism.

2.1 Sets, types and tokens.

Chomsky's generativist enterprise was originally instituted and remains on the foundations laid by an assumption, precisely the assumption (the metaphysical realist's assumption) of the always and already of the linguistic (recall that this creed involves there being "a totality [that is] fixed once and for all"):

> Assuming the set of grammatical sentences of English to be given, we now ask what sort of device can produce this set.[4]

As we have previously noted, this set is standardly and (as we have also noted) problematically understood as a set of types related - here is the problem - to the

products of actual linguistic behaviour in terms of the Peircean relation between type and token. However, the privileged member of the polarity is resolutely the abstract type (and it must be abstract by virtue of being real independently of any instantiation; it is already "given"):

> one thing that [language] certainly does not consist of is individual linguistic events or utterance tokens, ... Instead, what is known in common, ..., is the system of linguistic types.[5r]

The problem is that this type/token manner of thinking results in certain anomalies:[6] firstly, the attempt to privilege the type over the token is questionable because each side of the polarity is defined by its mirror term; you cannot make sense of the notion of some concrete event being a token of something without having it being the token of a type, (i.e. the something which individuates it as a token of that something,) and you cannot individuate types (i.e. specify which one you are talking about) unless you token it. Moreover, once you introduce type-token thinking, the point of which is to elucidate the relation between the events of linguistic performance and the "given" set of linguistic expressions, one is caught in the bind of certain "internal perplexities".[7r]

To recap, types, being abstract and timeless, do not occur and do not have parts, so certain ineliminable properties of sentence tokens, (e.g. being temporal and having parts, parts which seem to be linguistic entities with linguistic properties, e.g. constituents,) cannot be predicated of the privileged and ontologically prior sentence type of which the token is the instance and where it is the type that is supposed to be explicating the nature of linguistic phenomena. But then we seem to have to reverse the priority, because it is the tokens and only the tokens that have certain of the properties that linguists are standardly interested in. We do not get to understand tokens by reference to their types. However, it is just this system of abstract types that is supposed to be given and is supposed to be the object of the linguist's enquiry offering us an understanding of the nature of individual instances. But if certain and criterial linguistic properties are only to be predicated of actual tokens, then this undermines precisely the initial realist assumption with its ineliminable abstraction for linguistic entities and which abstraction is articulated in terms of a set of types. The upshot is that we would seem to have to take, as we have suggested but coming from a different direction, an instrumentalist view of the linguist's abstractions/types: it would seem that "what is abstract in linguistics is not of-language, but merely of-and-

for-the-linguist, having a (merely) theoretical utility".[8r] Moreover, even supposing this can be patched up so as to maintain the proper predication of linguistic properties vis-á-vis the abstract members of the set of sentences, something is still awry, for precisely what we do not have is an account of how the posited object of study is related to the first order phenomenology of the linguistic, i.e. the products of linguistic behaviours: what we get is the "properly" linguistic being abstract east, and the actual behaviour being concrete west and the two seemingly fail to meet.[9]

The upshot, as has been severally observed (see the quotations below), is a species of solipsism: the object of study, the set of a-temporal abstract types, which is at the centre of the generativist enterprise, cannot be contentfully related to any behaviour. This is not just a matter of an empirical failure to provide any content for the MRG hypothesis - as has been our concern in previous chapters - but is precluded by the logic of the conceptualization that institutes the generativist's discourse about the linguistic: between the actual/concrete and the abstract there are no common denominators; the former into the latter, the latter into the former simply will not go; as Burton-Roberts remarks, the generativist's object of study, by virtue of its abstraction, offers "no E-scape". Other commentators offer similar observations:

> Modern linguistics has failed to give a coherent account of [the] process of realization, ... Ironically, the belief that speech is made up of instanced forms precludes our giving a coherent account of the central notion, that of the instance itself. If we take the abstract view of linguistic knowledge, we have no way of linking the abstract to the concrete.[10r]

> Chomsky has constructed a formal model, a 'mathematical' grammar which generates linguistic expressions or 'sentences'. But what is the relation of these linguistic expressions or 'sentences' to our everyday [linguistic activity]?[11r]

This question has real point, for without such a relation it is not clear that the selected object of study would be any more than an artefact of theory, or of some thinking, and an artefact that answers to nothing in reality; in other words, a fiction. This is because the resulting theory would not be explanatory of, because would have no contact with, any apparent and verifiable set of phenomena that we come across in the world; it would be, in short, (and this is where we return to the notion of a skeuomorph) a calculus that models no more than that which any calculus

ineliminably models, namely, itself. Furthermore, it would be incorrigible by any properties of any concrete actuality (recall our remarks regarding unfalsifiability, empirical strandedness and the incorrigibility of level one functions): if what we are concerned with is a system of types, with the properties that are proper to them, then many properties that pertain to tokens need not be relevant to the falsification of any theory of the types.

What is being remarked on in these observations is that the abstraction of the designated object of study (this itself resulting from the perceived need to go beyond corpora to include non-actualized possibilities) gives rise to the apparent need to conceive of the object of study as a set of sentence types. This involves the Chomskyan project in certain conceptual tensions. On one side of the coin, we apparently lose contact with the everyday phenomenology of the linguistic, that is with any actualizations/realizations (i.e. precisely what psycholinguists complain about); on the other side (same coin), there is the explanatory need to supply the linguistic with a psychological and so concrete locus while negotiating the abstraction of the object of study. It appears, however, that the designated object of study, the set of always and already sentences of a language, provides an ineliminable obstacle to a psychological (and so explanatory) ontology for the linguistic: the abstraction of the postulated object of study simply cannot be coherently negotiated in this way.

2.2 Infinitude and the linguistic.

Katz (1996) observes that the psychologism of Chomsky's project is an attempt to negotiate and naturalize the apparently inalienable abstractness of the generativist's object of study in terms of a concrete/cognitive/neurological ontological locus.[12] He also observes that this attempt is flawed: such a conceptualization, he argues, does not and cannot "allow for sufficient abstractness to provide a satisfactory interpretation of grammars."[13] The essence of Katz' point (see also footnote 27, chapter III and Carr, (1990, pgs. 42 - 43)) is quite simple: the infinitude of the linguistic is incompatible with the finitude of the psychological (for remarks on the bold faced - indeed, positively brazen - "knows" see the footnote).

> Since Chomsky is taking a generative grammar to be about something mental/neural, and since an English grammar is about the denumerable infinity of English sentences, ..., it follows that those sentences must also

> be something mental/neural. Hence, there has to be a denumerable infinity of mental/neural objects. But given the finiteness and discontinuity of matter, ..., there can't be an infinity of mental/neural objects.
>
> The moral is simple. Infinity and abstractness go hand in hand. No concrete [e.g. mental/neural] interpretation of grammars can satisfactorily represent the infinity of sentences of a natural language. ... Consequently, the only way to obtain the unactualized possibilities required to do justice to what the field of linguistics ***knows*** about the sentences of a natural language is to non-reductively take that scientific knowledge to be about types. But, since types are abstract objects, taking theories in linguistics to be about types is linguistic realism [i.e. platonism].[14]

Chomsky's manoeuvre, the E-language/I-language distinction, is to disclaim commitment to the extensional psychological reality of a set of sentences: "sets are not in the mind"[15r] Rather the commitment is to a finite set of rules and parameters, a set which is perfectly compatible with the finitude of the mind/brain. As Katz remarks:

> The hope is that, being committed to only ... a finite system of sentence-generating operations which characterizes a potential infinity of sentences, linguistic conceptualism will be consistent with the finiteness and discontinuity of matter.[16r]

Ostensibly, again as Katz remarks, and has Chomsky has also suggested,[17r] this move places the generativist project in line with Brouwer's intuitionist/constructivist project for mathematics. The trouble is that, if this is the only way to preserve the psychologism of generativism (Katz suggests it is) and if the Brouwerian option is what we want, then what we want is a linguistics that is committed to the notion of the infinitude of the linguistic as no more than potential, as opposed to the Platonist commitment to an actual infinity. This is the point of defining the set of sentences intensionally as opposed to extensionally. However, this is precisely what we do not get: the actual practice and the thinking of linguists in the generativist tradition is pervaded by the assumption of a metaphysical realist (Platonist) and actual, always and already construal of the set of theorems (i.e. sentences of a language) that are generated by some finite set of axioms. Indeed, Katz suggests that this cannot be avoided: if one wants to insist on a potential infinity, and generativism does, then:

> to say that such operations 'can' generate an infinite class of sentences, the explanation [of what this means] has to use 'sentence' in the type sense or has to use the notion of non-actual possibilia ... .[18r]

But this is to understand the non-actual as actually, even if abstractly, always and already, which is precisely what the manoeuvre was supposed to obviate.

The inappropriateness of characterizing the Chomskyan scheme as parallel with constructivism in mathematics emerges from the observation that there is no obvious room in a linguistic version of constructivism for a distinction between competence and performance. Precisely what a constructivism insists on, if we preserve Chomsky's terminology, is "a blurring",[19r] indeed, a conflation of the two poles of the competence/performance distinction. The whole point of constructivism, hence the name, is to excise commitment to objects unless they are actually constructable: it is their construction which is their own verification, it is how they come into existence. What exists is not some prior reality to which an event is in correspondence, rather there is only the construction, the mental event.[20] There is, from this perspective, literally no sense to the notion of a mathematical/linguistic object that has some always and already, even if abstract, existence independently of its actual construction:

> Where the realist says that every potential infinity presupposes an actual infinity, the constructivist replies that every potential infinity presupposes an operation and entails that there is no objective reality. What is true [or existent] is so only in virtue of our ability [a practical/performance-type ability] to apply the operation to a successful outcome, not in virtue of any correspondence with any such reality. ...
>
> [Where, on the one hand, the constructivist does,] the realist does not take the notion of construction seriously, as imposing any real constraint.[21r]

Indeed, nor does the generativist linguist, for such constraints as are concomitant with a constructivist perspective are in conflict with the "full generality - the notion of 'any sentence' - on which the [generativist] linguist relies."[22r] Here, of course, "any sentence" means any one of the set generated by the competence grammar. From a constructivist perspective, the focus is not on a level one/competence theory, rather

the constraints mentioned above, constraints which are intrinsic and properly constitutive, are those of a level two/performance theory:

> The entire significance for the mathematician of rendering more precise the concept of algorithm emerges ... in connection with the problem of a constructive foundation for mathematics.[23r]

A corollary is that constructivism entails that there is no sense to the notion of a sentence being in a language where that sentence is not only unactualized but also, because perhaps it is simply too long or defeats the parsing mechanism, unactualizable. The inclusion of such sentences in a language is, however, precisely the position to which Chomsky's level one/competence theory and standard practice are committed: taking such considerations as length to be proper to the object of study would be to confuse acceptability with grammaticality.[24]

As further grist to the mill and contention that a constructivism is incompatible with Chomsky's position (and illustrative of Katz' contention) are some more recent remarks by Chomsky. It is not a constructivism, with its concomitant anti-realism, but only a realism with its commitment to an actual, pre-existing, albeit abstract, infinity that makes sense of Chomsky's comment that:

> With only a slight air of paradox, we may say that languages, as such, are not usable. If some expressions are not parsable, as is often the case, they are simply not used, and the language is no worse for that.[25r]

It would seem one can only predicate (un)-usability of something which is in some sense already, of something which one is prepared to quantify over irrespective of whether it can be constructed/actualized. In short, Chomsky's (and generativism's) thinking is shot through with a metaphysical realist conception of the linguistic domain, with the linguistic as a set of always and already abstract types. In this respect, the E-language/I-language distinction is little more than cosmetic: while it might apparently offer to ground a constructivist view of the linguistic, it is a promise that is not met precisely because it does not integrate the actualizing constructive procedure into the object of study. Furthermore and crucially, the price of not meeting this promise is, if we accept Katz' contentions, a failure to preserve a psychological ontology for the natural linguistic, for what we are referring to, the set of sentences in the language unconstrained by performance factors, simply has to be

construed as constituted of abstract objects, many members of which exceed the limits of a psychologist ontology. The conclusion that falls out from these remarks is that there is no ontological hope for the generativist object of study except by way of a Platonism/autonomism.

The internal tensions that arise from an attempt to construe this object of study as psychological come out further if we inspect a little more closely Chomsky's "slight air of paradox". On the one hand, the generativist cannot cogently (although does) think of the members of the generated set as existing independently of their construction, i.e. extensionally, not if he wishes to think of them as having a psychological ontological status (they are too many and too many too long). Consequently, he has to (even if he habitually does not) think of the generative set as merely potential, as specified in intension. But then, what sense is there to "potential" where many members in the extension of the term (i.e. many members of the set generated) are, as a matter of psychological fact, not potential at all, but are, in this actual world, unusable, i.e. impossible because they cannot be actualized? But, this seems to commit the linguist to saying that certain sentences of a language are, like all the others, potential but, unlike others, not potentially potential ("see how high the seas of language run"). This puzzle attempts to be accommodated by "cannot" being taken to be linguistically contingent, a matter of performance, as, in Bertrand Russell's phrase, remarking on "merely a medical impossibility". But, is not this "medical impossibility" no other than a psychological impossibility. What emerges from this attempt to maintain a psychologism is a distinction, and, in the event, a seemingly unreconcilable tension and breaking of contact between two sets of facts: between, on the one hand, the linguistic (supposedly psychological) and, on the other hand and rather peculiarly, also the psychological.

This might strike one as odd. It is an oddness that, to be negotiated, instigates, as we shall see, the recourse to the familiar numerous set of intruding other factors to "explain" the apparent bifurcation. However, this itself has a consequence, parallel to the one we have remarked on and equally questionable: one set of cognitive facts - knowledge of I-language, by which knowledge we are supposed to be able to recognize licensed sentences as licensed sentences - parts company from and is privileged over (in the jargon, is potentially verification-transcendent of[26]) another set of cognitive facts - the facts about which set of sentences we do, in actual fact, recognize and which we can, in fact, actualize. (We take this up in detail below).

However, might one not ask just which psychologically real language it is that is not usable, that is not psychologically realizable? Is it the case that the language we

"must speak [,] the language of everyday ... is somehow too coarse and material"[27r] to answer to the real linguistic facts, those linguistic facts that generativism is in answer to and seems to somehow know antecedently, which are always and already, in excess of and elsewhere than linguistic events? What other language is this which is always and elsewhere, which we know but which is unusable? For the unusability of a language (and its exceeding, as it must do, our intuitive recognition) is entirely consistent with, is indistinguishable from, that language not being the case in respect of any psychologically realized or realizable human language. A solution (Katz' and Carr's) to these apparent antimonies is to maintain the realism but drop the psychologism on the grounds that this is the only way to coherently accommodate the properties of the standardly accepted object of study, which object makes sense of the working practice of linguists.

However, is it not at least possible that what generativism is answering to, in answering to the non-actual, is no more than an idea and a fiction of the linguistic, the "non-spatial, non-temporal phantasm" of Wittgenstein's phrase? This is not to question the objectivity of this unusable language, it is surely an objective one, but objective only in the way that a formal, mathematical system (i.e. a generative grammar) and its set of strings are real; that is as *abstracta*. It is, however, to question the reality of the relation of correspondence between those abstract entities and some physical or psychological *illata* that are actually existent. Such suspicions would at least cast some resolving light on the paradox that Chomsky notes. There is, after all, not even the slightest air of paradox in saying that a language that is a fiction and an artefact - a skeuomorph - is not usable.

2.3 Infinitude as skeuomorph.

> *We predicate of the thing what lies in the method of representing it. Impressed by the possibility of comparison, we think we are perceiving a state of affairs of the highest generality.*
>
> (L. Wittgenstein[28r])

Central to the misgivings raised in the above is the putative infinitude of natural language. It is this which induces the air of paradox. Infinitude is not a property that admits of empirical confirmation. One might ask how it gets to be so confidently predicated as a property of natural language (compare our quotation from

Katz, pg. 219 and the corresponding footnote). As it turns out, it is so predicated only by a sleight of hand which involves circularly assuming properties of what is supposed to be disconfirmable, i.e. the formal model, the theory-constitutive metaphor that provides epistemic access, that, in Saussure's phrase, "creates the object".

Smith and Wilson provide an example of the standard reasoning. On the basis of the acceptability of multiple right embeddings (e.g. "I said that you knew that you felt ...") they argue that (my italics):

> As there is no *linguistic* limit to the number of times S can be reintroduced ... it is impossible to construct the 'longest' sentence in English, and hence the number of sentences generated is infinite.[29]

This also provides the rationale for the inclusion of multiple centres within the language: the grammar has to account for acceptable sentences such as:

> 1) The rat the cat ate died.

The case of multiple right embeddings argues for unlimited recursion, consequently, multiple centres cannot be excluded from the set of grammatical strings, despite the fact that nobody (except perhaps linguists) has judgements of acceptability for sentences of any greater recursive complexity. This amounts to the claim that such sentences are verification transcendent. The same reasoning is applied to the case of strings that are judged acceptable but which can not be included within the set of sentences in a language. Smith and Wilson cite in illustration (here slightly amended):

> 2) That is the sort of book that, once you have read, you really feel you want to give it to all your friends.

This is to be excluded from the set of grammatical sentences on the basis that:

> If we incorporated the principles used to form [it] into English grammar, they would immediately give rise to the clear ungrammaticality of:
>
> This is a book which I gave it to my friend.
>
> In other words, whatever it is that makes [the former] sound natural, it is not, and *cannot be*, a linguistic rule.[30]

However, what is questionable in the above quotations is the use of "linguistic", for the reason that the linguistic (and what "cannot be" the linguistic case) is being assumed and defined as corresponding to the parameters and properties (in the first case, unlimited recursion) of the formal apparatus, the formal system which it is supposed to be the business of enquiry to test as regards its fitness as a model of the linguistic. Moreover, it seems that one is released from empirical constraint, for what is the theory-external evidence (external to the assumption of a generative model) that multiple centre-embeddings (or indefinitely long sentences) are to be accounted for as being included within the set of sentences of a language except for the fact that they have to be there as a reflex of the realist interpretation of the formal system that is applied to in the task of furnishing an explanation? This move, to be justified, appears to require the Galilean assumption of the literality of the formal system metaphor, that some formal system prefigures the properties of linguistic objects and that the properties of the formal system inhere in that object. In other words (Wittgenstein's), such formal models constitute "a preconceived idea to which reality *must* correspond" (compare "must" with the force of Smith's and Wilson's "cannot" - both are seemingly of sufficient force to override any mere actuality).

A rejoinder might rehearse the reasoning that we can be confident in asserting the grammaticality of impossible to be actualized sentences (e.g. multiple centre-embeddings) by deducing that they must "go on in the same way" as perfectly acceptable sentences, i.e. sentences of which we have perfectly adequate evidence and in answer to which the property of recursion was posited in the first place. The fact of recursion in the attested cases suggests a general rule, the attested cases are as if "a visible section of rails invisibly laid to infinity."[31r] However, when we think in this way, what we are doing is, figuratively, putting on the glasses as provided by some second-order thinking *about* the linguistic; we are taking up some meta-perspective as provided by the conceptual apparatus which we bring to theorizing and we are thinking through that - on those rails - to the not yet actual, to what must be the case, i.e. what is predicted *if* the formal system is true, i.e. is the intrinsic condition, of the linguistic. What excises the conditional is the Galilean assumption that natural language is a formal system. What we have is, from the I-language perspective, the linguistic-as-formal-system and, from the E-language perspective, the linguistic as the objects, i.e. sentences, that interpret that formal system. These objects, however, are never to be come across in this actual world, they are abstract types, nor, however, are their tokens to be encountered, because they are not-actualizable, but this latter empirical fact cannot, nor, indeed, need any such fact, impugn the transcendent

logical fact. It would seem that linguistics is not an empirical science, but is, by fiat of the originary theory-constitutive metaphor, a formal science *simpliciter*.

One might still be tempted to cling to the empirical legitimacy of Smith and Wilson's "must". However, the force of this "must" not only carries one out of any empirical realm toward the making of assertions that are verification transcendent (i.e. unable to be borne out by any data), but also one is carried toward the making of assertions that are frequently contradicted by the empirical evidence: speakers simply do not have judgements of acceptability of, for example, multiple centre-embedded strings. It is at this point that evidence is supplemented and interpreted by the "intrusion of numerous other factors". As we have discussed, it is by no means clear what the evidence for such other factors is other than the theory-internal tensions that require them to be the case so as to preserve, and make unfalsifiable, the realist reading of the theory, i.e. the literal construal of the formal system metaphor. In short, what we get is indeed the formal system as a "preconceived idea to which reality *must* correspond", some "superlative fact" which when not borne out by any evidence, i.e. speaker judgements, is sufficient (care of the intrusion of numerous other factors) to override and confute that evidence. However, this is not so much theory-saving as theory-confounding, for what emerges again is the difficult to make coherent bifurcation of supposedly psychological facts: the I-linguistic on the one hand, and, on the other, intuitive judgements. What is odd is that the latter are supposed to be informed by the former, for this is the very rationale for taking such judgements as relevant at all, as being data about that I-language. What results from this bifurcation, as we detail in the next section, is the undermining of this rationale and with it an undermining of both the mind-internal ontology for the I-linguistic and also of the ability to provide an explanatory linguistics on the basis of a competence grammar.

2.4 Intuitions revisited: bivalence as skeuomorph.

The "preconceived idea", the formal system metaphor with the metaphoricity illicitly excised, as we have suggested previously, constitutes the axis of the idealization and abstraction that is deemed a methodological requirement for linguistic enquiry. It is what modularizes the gross phenomenon. What results is a skeuomorph: the object of study is made to answer to (and the data are tidied up on the axis of idealization so as to answer to) the properties (infinitude and bivalence) brought in by way of the conceptual apparatus that institutes the discourse and "creates the object of

study". Where those properties are not borne out by any evidence, we get the citation of "the intrusion of numerous other factors" to account for this empirical shortfall. But all this amounts to is a wielding of Occam's broom justified on the grounds of simplifying the theory. However, this simplification (recall simplicity is definable only in theory-internal terms) is no more than a case of assuming that which is supposed to be open to falsification and thereby making it unfalsifiable. What also results, and which is the focus of our concerns in the following, is an undermining of the rationale for positing the I-language in causal explanation of the ability to have intuitions, as that which provides the causal basis of our linguistic ability. In short, what is brought to question is the status of the I-language as explanatory of the speaker's linguistic ability.

In brief, the point is that if it is claimed that there are grammatical sentences that are not recognizable, by a speaker's intuitive judgement, as acceptable (or ungrammatical sentences that are judged acceptable, or, indeed, sentences that apparently violate the principle of bivalence, i.e. are on some occasions deemed the one and on other occasions the other), then, and this is the difficulty in the position, it seems that one has to maintain that there are certain sentences that are licensed by the I-language, as a putative psychological fact, and so are what is known, but which the I-language which we putatively access in making such judgements, does not recognize. It seems we have both to know a sentence to be in the language (care of the I-language that licenses the sentence) and, at the same time, to not know that a sentence is in the language (care of our intuitions, themselves putatively care of the I-language that licenses the same sentence).[32]

The "other factors" are what are posited to get round this anomaly, or, to be more precise, get in the way of access to the I-language. However, this does not solve the problem because, the strong whiff of unfalsifiability apart, what is then brought into question is the very cogency of taking intuitive data to be evidence for the content of the I-language at all: if, in certain cases, it is allowed that there is a discrepancy between what is judged acceptable and what is licensed by the grammar, then, in these cases, that which is the basis for the judgement, the information that is accessed, cannot be the I-language. So, in these cases the judgements are data about, and are data that are informed by, the "other [i.e. performance] factors". To explain the ability to have these intuitions one cannot postulate the I-language, something else must be the informational cause of the judgement. But then, the I-language was posited in the first place to explain, amongst other linguistic behaviours, the ability to have intuitive judgements. This is what makes sense of taking such judgements as data. Now we

have to say that we can, indeed must, have such judgements on some other basis. But, does one need to postulate the I-language, defined as a context-independent autonomous body of declarative knowledge, as being the basis for *any* intuitive judgement, and, by parity, as being causally implicated in any linguistic behaviour, as being, in any contentful sense, psychologically real? Might it not be the case that it is precisely the numerous other performance factors that are properly, to some crucial measure, constitutive of speakers' linguistic ability? Moreover, if linguistic behaviour vis-à-vis intuitions of acceptability need have no recourse to a body of autonomous linguistic knowledge *qua* I-language, one might ask just what is the I-language hypothesis explaining, other than linguistic facts that are not actual, i.e. not recognizable as, linguistic facts. Just what verification transcendent language is this that we are talking about (and *not* speaking) and just what work is it doing other than allowing the theorist to get psychologically real about his grammar?

To unpack these remarks: the rationale behind using intuitive judgements as data (recall, Fodor reminds us that we need some story to relate the data to that which they are data about) is that they are taken to be informed by the I-language and, hence, provide the linguist with evidence of what strings of a language are members of the set of sentences generated by the grammar; ostensibly they make the theory *qua* grammar falsifiable. If the I-language is what we know when we know a language, and the I-language is the knowledge resource that is accessed when we have intuitions, then it is predicted, all things being equal, that speakers should be able to recognize as acceptable all and only the sentences generated by the grammar, for this is what makes sense of the practice of using intuitive judgements as potentially disconfirmatory of theoretical claims. This is how linguistics gets to be a fully paid up member of a Popperian natural-scientific club.

However, an unqualified interpretation of acceptability judgements as being directly disconfirmatory of the content of the I-language has to be modified because, simply, many strings that on a generativist model have to be taken to be licensed by the grammar are not recognized as acceptable sentences of a language by speakers of the language. Examples are sentences that are, in general, recursively complex, as in:

> 3) The man who has knowledge of the fact that the Prime Minister knows of the whereabouts of the leader of the opposition's secretary's files is under arrest.[33r]

Other systematically problematic sentence types are certain garden path sentences that on some occasions, i.e. in certain contexts, are, and on other occasions are not, judged

acceptable.[34] If such judgements were allowed to be taken at face value (i.e. as data directly about the I-language), then this would bring to question the principle of bivalence which is supposed to be applicable to natural language strings: they either are, or are not grammatical and are so antecedently to their tokening. (Bivalence is a direct corollary of a metaphysical realism, it comes out of the assertion that there are, antecedent to any actuality, a defined set of determinate states of affairs, in the linguistic case, a given set of sentences; any string either is or is not a member of that set, there is no middle term.)

Relatedly, one might also mention the frequently attested cases of native-speaker informants who, individually, have an unhappy habit of giving conflicting judgements on different occasions about identical sentences - one might think that they do not know (in the theoretically approved sense of "know") their own language. It is just as well we can rely on linguists' "armory of techniques" to sort out what their judgements should really be. Of course, it might be pointed out that the need for this complexity (or, to give a certain rhetorical spin to the same phenomenon, this licence for any *ad hoc* undignified shuffle) can be traced back to, and so bring to question, both Chomsky's initial realist assumption of a language as a determinate, already given set of sentences and also the correlative carving of the gross phenomenon by a distinction between competence and performance.

A consideration that is put forward to argue for the maintenance of this carving of the gross phenomenon is that we need to maintain the standard distinction between competence and performance (and so maintain a realism in respect of the competence grammar as a mentally represented autonomous system) because it answers to certain observed facts. This is the line of (standard) argumentation taken by Higginbotham.[35] He argues that there is need to posit the competence grammar as a sort of "clerk", "a context-independent cognitive resource" who is consulted from time to time on matters linguistic, in order to account for the fact that, in certain cases, for example garden-paths, speakers are able to reflect on initially unprocessable strings and come to the conclusion that they are in fact acceptable sentences of the language as licensed by the grammar. The picture we are encouraged to have is one of the intruding, performance factors being suppressed and so allowing some direct access to what our linguistic clerk has to tell us.

This is not, perhaps, an impossibility (I am being charitable), it is, however, both unlikely on the one hand (because difficult to make coherent,) and, on the other,

wholly insufficient reason to ground the conclusion that we need an autonomous and encapsulated body of linguistic knowledge to account for this ability.

To take this latter point first: in the case of multiple centre-embeddings it is standardly argued that the difficulty of such constructions is to be placed at the door of the perceptual strategies that the parsing process implements. Higginbotham's suggestion, which is in line with Chomsky's comments,[36] is that, given some time to reflect, a subject can suppress these factors. But, even where a subject does accede to such a sentence's acceptability, there is no very good reason to privilege the supposition that this is because a body of autonomous linguistic knowledge is being made use of. In any case, it cannot be applied to because its information is putatively tacit. The general point is that, given time to reflect, then, while this may enable "extrinsic constraints" such as memory limitations to be suppressed, precisely by the same measure, this means, because we can not apply to our tacit knowledge, that the extra time allows other, i.e. *extrinsic*, cognitive abilities to come to the rescue. These extrinsic factors are putatively irrelevant to a string's grammatical status. They are, however, seemingly crucial to the verification of its status and to a string's being constructed as a psychological object for contemplation by our clerk. What emerges, if we insist on this view, is the position where psychology bifurcates into two sets of facts, however, one of these sets of facts, the linguistic, are transcendent of those facts which are crucial to their verification. Not only is this somewhat unsatisfactory, looking like a Platonism under the guise of an ersatz psychologism, but it also turns out to be very difficult to make coherent.

In illustration of the role of extrinsic factors, as we noted above, one might persuade a speaker (or a speaker might persuade herself) that, given the acceptability of a sentence with one centre-embedded clause, and given unlimited recursion in the case of right-embeddings, then a sentence with two (or unlimited) centre-embedded clauses must also be grammatical. But what gets us to this conclusion is a non-linguistic-specific ability for deductive reasoning. It is not, and cannot be, evidence of the nature of a speaker's linguistic competence, but of a speaker's ability to think logically about the string taking the unrestricted recursion of a grammar as a premise. It cannot be evidence for the reality of an internalized competence grammar because this premise cannot itself be derived from inspection of one's tacit competence, and this because that competence is precisely that, i.e. tacit.

More generally, it is by no means clear just how much meta-linguistic, or para-linguistic, knowledge might be brought into play when a speaker is asked to make a

judgement of a sentence's acceptability. By "meta-linguistic knowledge" I mean knowledge (or opinion) about language that one might pick up by virtue of being a speaker of a language or by virtue of being educated about a language, i.e. as distinct from the putative strictly *sui generic* and tacit linguistic knowledge that constitutes the final state of the I-language.[37] An implicit question here is just how naive is the naive linguist and to what extent does this knowledge/opinion intrude into the making of judgements? Such meta-linguistic knowledge/opinion, and so the possibility of its interference as an intruding "other factor", will be the more likely in the case of the educated speaker of a language, and yet more so in the case of the linguist (and it is standardly this latter, of course, who supplies linguistic data). More to the present point however, these considerations suggest that there is little mileage to be gained in the attempt to argue for the psychological reality of a grammar as a "context-independent cognitive resource" on the basis of an ability to reanalyze hard to process sentences. Of course, one might want such to be the case, but wanting it so does not make it so.

To take up our other point: it is unlikely that extraneous performance factors can be suppressed because "what you have to remember about parsing is that basically it's a reflex."[38r] The point about a reflex is that it is automatic and cannot be suppressed nor circumvented in the way that Higginbotham seems to require. Moreover, it is not at all clear what it would mean for a judgement to be made directly about some linguistic object, on the basis of accessing an autonomous body of linguistic knowledge, before that linguistic object has been processed, for what would that judgement be about? It could not be about a linguistic object because, before they are processed, linguistic objects are no more than acoustic or graphic events and so no more linguistic than is the sound I am making typing this or the coffee stain on my desk. What we have to conclude is that if performance factors are suppressed, then there can be nothing for our clerk to have any opinion about at all. But does this not suggest that it is just these performance factors that are constitutive of individuating linguistic properties?

These observations do, however, suggest some account of what might be going on when we make some judgement. Feasibly, at least initially feasibly, what happens is that the parsing process results in some structured object to which our clerk either assents or dissents. This might explain how it is that certain sentence types (such as multiple centre-embeddings) are deemed unacceptable. Perhaps it is because the parsing process simply fails to provide a structured object for inspection and fails because it cannot handle certain levels of complexity. But, if this is the case, then how

does one get to explain how it is that perfectly processable strings that are ungrammatical are judged acceptable? The point is not restricted to, perhaps, one or two possibly debatable examples, but is, we are told, quite general:

> so called "ungrammatical" or "deviant" sentences are often quite readily parsable and are even perfectly intelligible, and quite properly used in appropriate circumstances.[39r]

And if they are parsable, then the parser must produce a structured object but, at the same time, one which, seemingly, fools our clerk. Perhaps, then, our clerk does not know what he knows when he knows a language, or knows it and does not know it at the same time. Or maybe he gets confused, or maybe we are doing it wrong and applying to the wrong resource and other factors are still intruding. But, if that is the case, how do we know when we are doing it right? Perhaps we need a linguist (with her armory of techniques) to tell us. But then, just who is it that has an entitlement to views about a language, just whose data and which data are directly valid, which language is it that we are talking about?

To take a different tack: if our speech is littered with strings that are both "properly used" and acceptable, while at the same time their "deviancy" consigns them to the technical status of being not in the language, then again one may ask what language is it that we are speaking, and which is outside of the proper object of study? For it is, seemingly, one that we know by virtue of knowing something other than a language, where "language" is defined as an I-language. But, if we need something other than an I-language to explain this language, just what is it that an I-language *is* explaining; do we need to posit an I-language that is autonomous of performance to account for our ability to have *any* intuitions of well-formedness? This is, of course, what makes sense of the practice of using intuitive judgements as data in the first place. Chomsky remarks that it is in fact no more than an "assumption or pretence that ... informant judgements give us 'direct evidence' as to the structure of the I-language",[40r] but might not the pretence, which we might think of as that which is potentially fictional, be the psychological and putative causal status of the I-language? Precisely grounds that might be cited for its psychological/causal fictionality are, if we insist on the use of intuitive data, the difficulties involved in making coherent an account of how this I-language is related to intuitive data, how it is that it can be consulted. This is at the centre of the problem, for it seems that in positing a body of

tacit knowledge in explanation of the forms of linguistic structures, that knowledge is taken to be blindly applied and impossible of conscious access. Yet the point of, and justification for, taking intuitions as data is that this same body of knowledge is also available to be accessed by, and is "visible" to, some subpersonal system, our "clerk", independently of the blind following of the rules, which rules constitute the body of tacit knowledge. It is a similar point that is made by Wright in respect of explanations in general that make use of tacit knowledge.[41r] Higginbotham summarizes Wright's contentions as resulting in a dilemma:

> either familiar talk of rules or principles of language internalized by native speakers should be understood as metaphorical merely; or else we must admit unconscious rule following, a conception that faces the task of explaining how a rule can be blindly applied and at the same time something that the organism heeds.[42r]

And, we might add, something that the organism is on occasions incapable of heeding, those occasions being those where the data from intuitions are at odds with what should be the case if such judgements were to directly reflect the properties and parameters of the formal models which articulate Chomskyan linguistic theories.

In sum, it looks suspiciously like the "intrusion of other factors" with the correlative "armory of techniques" is no more than a means to interpret the data in order to maintain the correspondence of the parameters and properties of such formal models with those of some putative, performance-autonomous, psychological system. The invocation of these other factors is the condition on maintaining a psychological realism for the I-language together with the assumption, correlative of the realism, of bivalence: that the determinate set of determinate sentences of a language is already given antecedently. It is plausible to suggest that what is happening is precisely an instance of some idealization/creation of the object of study along the axis projected by the theoretical apparatus that instigates the discourse. Bivalence, at the level of sentential syntactic objects, has to be the case because it is a requirement of the formal system metaphor, which formal system generates precisely a determinate set of sentential syntactic objects. Bivalence is, in short, a skeuomorph.[43] What the supposedly innocent, and ostensibly methodologically necessary, idealization, by way of application of the "armory of techniques", is quite plausibly doing where it addresses the aforementioned vagaries is no more than packaging and falsifying the

data so that they can fit into the predetermined patterns as required by the formal system metaphor. The evidence for the fictionality being none other than the indeterminacies of the primary data.

However, the rationale that provides a justification for the interpretation of data as giving access onto the psychologically realized competence grammar results, on the one hand, in the dilemma, amounting to the anomaly, if not incoherence, which we have observed, while, on the other hand, the attempt to negotiate the discrepancy between what is required by a realism (a given and determinate, denumerably infinite set of sentences) and the actual data (which, *prima facie*, is compatible with neither the infinitude nor the bivalence of the linguistic) results in the need to posit the intrusion of performance factors to counter the bifurcation and loss of contact between the two sets of putatively cognitive facts: those of what our intuitions tell us and those that our intuitions ought to be telling us if they were to reflect the informational content of the I-language. But, all that this recourse to other factors does is to undermine the interpretation of the data as being in some sort of relation to a psychologically realized competence grammar; this because it brings into question the explanatory role of the I-language (knowledge of sentences defined on a syntactic axis) as an autonomous locus for a speaker's knowledge of language. For once such "intrusion" is admitted of in intuitive judgements, then, by parity, it becomes perfectly cogent to canvas the possibility that these (performance/pragmatic) intruding factors are crucially involved in, and constitutive of, linguistic ability in general. What obtrudes, if we follow this reasoning, is the possibility that those properties that individuate linguistic entities are always and only to be found in actual linguistic events; it is not so much that other (performance) factors intrude, as that they constitute the linguistic phenomenon. To use a theological metaphor, the linguistic is not so much begotten, as made. Such a possibility would shift the explanation for the individuating properties of linguistic entities, at least in some part, onto the systems that are involved in performance, a move entirely in consonance with a constructivist emphasis on algorithm and the actualizing cognitive operations: it would be the algorithms that are to be accommodated as central to an explanatory account.[44]

To come to the same point somewhat more directly: it is simply not the case that an always and already metaphysical and psychological realism, with a correlative principle of bivalence, may be comfortably or naturally deemed appropriate to linguistic objects, not unless we accept the bifurcation of the psychological into the

non-transcendent and the transcendent. One may cite as evidence: firstly, the above mentioned apparent vagaries of native-speaker judgements, (they are only vagaries on the assumption of the autonomy of the linguistic from performance). Secondly, is the reported fact of the crucial role that context plays in determining acceptability judgements and interpretations in garden-paths and lexically or syntactically ambiguous strings.[45] In other words, if we follow Chomsky's line and identify what it is that confers on us an ability to have such judgements with what our inquiry is about, then we have to recognize the substantive role that performance and context plays in fixing the syntactic properties of natural language strings.

The other option is to accept the breaking of contact and the verification transcendence of linguistic objects, maintain the generativist's object of study, and to take, for the reasons cited (infinitude and the putative fallibility of intuitions), the realist and Platonic/autonomous road. Perhaps:

> We want to say that there can't be any vagueness .... The idea now absorbs us, that the ideal 'must' be found in reality. ... We think it must be in reality; for we think we already see it there.[46r]

And, indeed, we do already see it there as something to be answered to, and what we see has a certain objective content: what we see is an object that is the denotation of the everyday notion of a language. It is some such cultural and socio-historic artefact that, for instance, the Académie française talks sense about (I exaggerate); it is what we refer to when we refer, in our everyday way, to "English" or "Italian": an always and already compendium filtered through some *mélange* of socio-political and cultural concerns. It is in reference to such an object that one can sensibly produce a volume entitled "A Guide to Common Errors in English". But, just what is the nature of this entity, "English", that is referred to and to which, apparently, such objective content can be ascribed?

Cultural artefacts and institutions such as this notion of "English" are just as much objects as are any others.[47] Notably, and the point of this excursion, both languages, in this artefactual sense, and "the lexicon" are taken to be proper and primary objects of study for linguistics, by Katz, in the case of the former,[48r] and by Carr in the case of the latter (both are also not obviously distinguishable from the objects arrived at through the process of "abstraction and idealization" which is claimed to be a methodological requirement of an enquiry from Chomsky's "standpoint of individual psychology": again a blurring, if not erasing, of any

contentful distinctions between the ontological positions). Indeed, it is the incompatibility of the notion of a "lexicon of a language" with a perspective from individual psychology that Carr uses to argue, in tandem with the argument from infinitude, for the inadequacy of a psychological ontology for the linguistic:

> Given that, in the generative enterprise, we take knowledge of the grammar of a language to define what it is we know when we know a language, it is clear that we want to claim that the lexicon of a language is a linguistic reality. But that reality is not clearly an object of *individual* knowledge; it is not clear what it would mean to claim that each of us, as members of a speech community, possesses the lexicon *per se*. ... Rather, the [lexicon of a language] is more easily interpretable as a public object.[49r]

This is undoubtedly the case. And it is also undoubted (at least here) that such objects constitute valid objects for some enquiry: as Wittgenstein's remark, cited prefatory to this chapter, reminds us, "it is possible to be interested in a phenomenon in a variety of ways." However, it is not the case that such objects need be taken as normative, as some pre-theoretic and unreducible given, such as to define a necessary and only object of enquiry that has to be answered to in and of itself. Arguably, such objects (e.g. as correspond to these notions of a language or the lexicon) are constructions and idealizations out of our meta-talk and our thinking about linguistic behaviours; we might term such constructions second-order phenomena. As objects of study such constructions are within the province of the social sciences (widely construed). However, it is the first-order individual linguistic behaviours - along with the facts of acquisition - which appear to have the more obvious claims to be a primary phenomenon, for they are what there is meta-talk about. Indeed, also arguably, when we are seduced by the familiarity of the resulting artefacts into thinking them primary, it is at least possible that we get things the wrong way round and are answering to no more than some second-order idea and ideal of the linguistic constructed out of our generalizing over actual and individual linguistic behaviours.[50] (In the following section we adduce grounds for the fictionality of this idea/ideal.)

One might, however, speculate as to the extent to which this ideal and socio-cultural artefact feeds into (either as constitutive of, or supplementary to,) an individual's instantiated knowledge of language which is involved in the aetiology of linguistic behaviours. For example, consider how considerations relevant to a

description of Latin were transposed onto English such as to tell us not to ignorantly split infinitives and that prepositions are bad words to end sentences with. It is this artefactual language, up with which one can just about put, which that indefatigable writer of letters of complaint to the BBC, "disgusted" of Tunbridge Wells, appears to speak as a native and by whom departures from this standard would undoubtedly be judged unacceptable - that's why he complains so much. At this point the linguist appears and tells "disgusted" of Tunbridge Wells that he is mistaken as to his judgements. To which it would not be unreasonable for him to reply, in Chomsky's words, "What do you mean ... ? I am a native speaker of the English language."[51r] At this juncture, the notion of an idiolect comes to the rescue together with the reminder that enquiry requires a certain degree of idealization, i.e. a licence to ignore "disgusted" of Tunbridge Wells, in other words, a licence to accommodate any problematic data by idealizing along the lines suggested by one's competence grammar: the supplementarity (as opposed to the properly constitutive) nature of this "knowledge" is shown to be the case precisely and unsatisfactorily by assuming it to be supplementary. Its one of the "armory of techniques". (Of course, one gets to tell a coherent story; but then how could one not?)

Such observations raise, to be left as a moot point, the question as to what, if anything, constitutes the linguistic counterpart of the noble savage; the question of the extent to which normative social and meta-linguistic factors are integrated into, and are in some measure constitutive of, actual linguistic phenomena.[52]

3.0 Anti-realism.

3.1 Wittgenstein on language.

As we have previously argued, if a Platonism/autonomism is the only cogent ontological status for the object of study as postulated in Chomsky's generativist paradigm, then it is not so much a solution as the deferral of a solution: precisely what we do not have, and are precluded from having, is an explanation as to how the individual gets to be in any relation with language and so no account as to why human languages are as they are. Somewhat paradoxically, the attempt to provide a psychological and embodied locus for linguistic knowledge has left that body of knowledge peculiarly disembodied. What we are also committed to, as a requirement, in preservation of realist claims, is the verification transcendence of linguistic entities

and so the correlative breaking of contact between the set of abstract types of the linguist's concerns (defined either in intension or in extension) and the actual, this-world products of linguistic behaviour. (We are asked to imagine, or feign the existence of, as we can and do, the Cheshire cat's grin when the cat has disappeared.) Indeed, Burton-Roberts suggests that recognizing this bifurcation as the case, and so recognizing these latter as not being properly linguistic at all, makes better sense of ("is more consistent with") "Chomsky's thinking ... than some of Chomsky's own remarks."[53r]

What we have is a picture of a grammar, (in cognitivist terms a level one function,) as a "superlative fact". A fact that is incorrigible by any mere actuality, but which prefigures that actuality as its always and already possibility, and of which possibility that actuality is a more or less flawed copy: the ideal (in the sense of pertaining to the idea, the possibility - the grin regardless of there being a cat) is rather more real than the actual.

It is such a realist construal of some calculus, some set of rules, as an always and already idea, as some superlative fact that is questioned in the *Philosophical Investigations*. Wittgenstein's observations there offer a commentary on our preceding remarks and, interpretably, rehearse elements of a mathematical constructivist's anti-realist perspective for the linguistic.[54]

A calculus offers, as it were, a picture or a map of the linguistic. Equating the linguistic with the "machine" of Wittgenstein's discussion, the calculus figures:

> The machine as symbolizing its action: the action of a machine [,the "action" of the linguistic "machine",] ... seems to be there in it from the start. What does that mean? - If we know the machine, everything else, that is its movement, seems to be already completely determined. ...
>
> "The machine's action seems to be in it from the start" means: we are inclined to compare the future movements of the machine in their definiteness to objects which are already lying in a drawer and which we then take out. ...
>
> We might say that a machine, or the picture of it, is the first of a series of pictures which we have learnt to derive from this one.
>
> But when we reflect that the machine could also have moved differently it may look as if the way it moves must be contained in the machine-as-symbol far more determinately than in the actual machine. As if it

> were not enough for the movements in question to be empirically determined in advance, but they had to be really - in a mysterious sense already *present.*[55r]

It is this sense of the "already present" that is, as we have noted, the mark of a metaphysical realism: the rules are as if "rails invisibly laid to infinity";[56r] the rules define/license, in Putnam's phrase, a "totality which is fixed once and for all",[57r] and a totality which is, by virtue of its infinitude, independent of any actual exemplification. In the linguistic case, moreover, this totality is, for the most part, independent of possible data, either actual tokens or intuitions about those events/tokens - it is in this way that the rules and the set of objects they generate are somehow more real than any linguistic event, are, in other words, verification transcendent.

What results from the tendency to view the linguistic machine as symbol is a breaking of contact from the claims of mere actuality, as if this were not our concern, as if mere actuality were "too coarse and material". The "movement" of the "machine-as-symbol" is not any actual movement, the movement of any *illatum,* rather it is the *idea* of some movement, derived from the internal logic of the picture we have of the "machine", i.e. derived from the "machine-as-symbol":

> When does one have the thought: the possible movements of the machine are already there in it in some mysterious way? ... And what leads us into thinking that? The kind of way in which we talk about machines. We say, for example, that a machine *has* (possesses) such-and-such possibilities of movement; we speak of the ideally rigid machine which *can* only move in such-and-such a way. - What is this *possibility* of movement? It is not the *movement,* but it does not seem to be the mere physical [or, neuro-physical] condition for moving either .... The possibility of movement is, rather, supposed to be like a shadow of the movement itself. But do you know of such a shadow? And by a shadow I do not mean some picture of the movement - for such a picture would not have to be a picture of just *this* movement. But the possibility of this movement must be the possibility of just this movement. (See how high the seas of language run here.)[58r]

What this passage is in attempt to show, and what its rhetoric trades upon, is the distinction that is available between some object/event in the world (an *illatum*)

and our thinking of it in terms of some conceptual apparatus (which apparatus - Wittgenstein's "pair of glasses" - projects that object as an *abstractum*, as a prefiguring idea, as a type). What Wittgenstein is bringing to question is the prefiguring reality of such *abstracta* - "Do you know of such a shadow", "some non-spatial, non-temporal phantasm"[59r] that subsists, as it were, behind the event and which is the prefiguring, pre-existent possibility of the actualized possibility? The implicit answer is no; rather, it is the case that there are the events and "an urge to misunderstand them"[60r] in our talk *about* them when we take on some meta-perspective.

A homely illustration might be useful here: walking down the High Street I noticed, fluttering in the air, half a dozen or so very small kites. Approaching, I saw, next to the man selling them, a sign proclaiming "The smallest kite in the world". My first reaction was to pick up on the uniqueness assertion by way of the superlative and, considering the evident plurality of identical kites, to take the sign's statement as demonstrably false. But, of course, there was nothing false - at least I would have shown misunderstanding had I asked the man which of the kites the sign was referring to. For the referent of the sign, what made its uniqueness assertion true, was not the instances/tokens, but the singular idea that each kite actualized. It is this Platonic idea that, I take it, corresponds to the "shadow" of Wittgenstein's remarks. There is clearly objective sense to this "shadow", however, the crucial question is as to whether one can get to refer to this idea/type except through its tokenings, its actualizations: where is this superlative and prefiguring kite except in its instances; what is the real ground of this idea, its source other than in the actual? More particularly and crucially, how can one get to identify which idea it is that you are talking about; in other words, how can one individuate the idea, in respect of the properties that are criterial of its identity, except through its tokening? Is it not "logically impossible to mention the type without using a token"?[61] What such considerations suggest is a reversal or a collapsing of the polarity: it is not the case that there is some antecedent and prefiguring idea that is the possibility that licenses and explains the actuality, (recall, this is the manner of explanation in the cognitivist paradigm: to explain patterns of behaviour one must have already the idea/level one function generating those patterns,) rather the actual is the grounds for the idea; it is out of the actual, the individual event that the idea is constructed and constructable; that idea has no prefiguring and independent existence otherwise and elsewhere than the event. What Wittgenstein's remarks argue for is the wholesale exorcism of the prefiguring ghost from the machine.

It is worth noting that the position Wittgenstein is criticizing here is the position that he appears to have held in the *Tractatus*: "Every proposition must already have a sense [,every linguistic expression must already have its individuating properties,] it cannot be given a sense [its individuating properties] by affirmation."[62r] The position that Wittgenstein endorses (or "shews") in *Philosophical Investigations* is the diametric opposite: propositions only have a sense, only have "life" in and by the act of affirmation; language is its use.

With a little poetic licence these opposing perspectives are interpretably expressed by our prefatory quotation from Eliot ("Words move only in time" - language as a series of events - or "the end precedes the beginning" - language as always and already). These perspectives, symptomatic of the two means of theoretical access we have on linguistic phenomena, are roughly duplicated by what we have observed as the orthogonal/incommensurable relation between studies of linguistic processing/actualization (i.e. studies that are included in but not defining of the domain of individual psychology) and studies of linguistic knowledge (i.e. what we have suggested resists a mind/brain-internal ontological status).

A useful way, for our purposes, of figuring these two perspectives is provided by some comments by Lemmon. He remarks, *contra* Strawson's (1950) criticism of Russell's theory of definite descriptions, that:

> It is as though one were to say that we cannot speak of a gate as having a definite colour, because the same gate may have different colours at different times.[63r]

Alternatively, however, and *pro* Strawson (and Wittgenstein), one might say rather more problematically, simply by exchanging, in Lockean terminology, primary qualities for secondary:

> It is as though one were to say that we cannot speak of a gate as having a definite [shape], because the same gate may have different [shapes] at different times.

But just what phantasmic gate is this that has no definite shape? The question appears to be one of determining quite what it is, if anything, that is constant (always and already) in, for example, a word or sentence type across different occasions of use, and what properties, perhaps linguistically crucial and criterial properties, are to be

found only in, and provided by, the actualizing event. In other terms, this amounts to the question of how the gross phenomenon is to be carved/modularized.

3.2 A "Representational Hypothesis"[64]

Burton-Roberts' "representational hypothesis" is to the purpose of accommodating the observed breaking of contact between the set of linguistic expressions, as specified by an autonomous competence grammar, and the products of linguistic behaviour while preserving a realism in respect of that grammar's generative capacity. The proffered solution is that the concrete (acoustic or graphic) events that are actualized in behaviours be regarded as not strictly linguistic, but rather "physical representations of linguistic expressions"[65r] which are the para-linguistic means of identifying and determining which linguistic expression, as specified by the autonomous grammar, is being referred to.

The view we are encouraged to is that these concrete representations are so in much the same way as a painter's picture of some object represents, but is itself distinct of, that object; the object, with its properties, is not constituted by the picture; it exists and has its properties always and already independently of the depiction. In the linguistic case, what is represented by (para-) linguistic behaviours are the members of the set of linguistic expressions of which the individuating linguistic properties (i.e. the tuple including syntactic, semantic and phonological properties) are fully specified, one might say begotten, independently and timelessly by the grammar.

A corollary of this account is that ambiguities (indeterminacy and apparent violations of a principle of bivalence in general) are not viewed as a properly linguistic phenomenon, rather they are a property of the indeterminacy and vagueness of the para-linguistic representational medium, i.e. the concrete sounds/marks that do the representing. An explicit aim of Burton-Roberts' paper is precisely to counter the tensions induced for a competence orientated and autonomous linguistics by the apparent phenomenon of "pragmatic intrusion" or "context-dependence" where pragmatic/performance factors are seemingly required to fix, in Gricean terms, "what is said". His solution is to preserve the autonomy of the linguistic and with it a correlative principle of bivalence by hiving off this phenomenon onto the para-linguistic.

In more detail: the problem that pragmatic intrusion poses for a realism articulated by way of an autonomous competence grammar is that, on this competence view, linguistic knowledge, "knowing how linguistic expressions are individuated" is "a matter determined uniquely, for each complex expression, by the [autonomous] grammar":[66r] the individuating properties of linguistic expressions are supposed to be specified, "antecedently determined", by the grammar. However, in a string such as (Wilson's and Sperber's example), "refuse to admit them", the crucial individuating properties, the fixing of "what is said", can only be resolved in a context such as selects for the reading of "admit" as either "allow access to" or "own up to".[67]

> In short, (i) pragmatic, conversational principles are supposed to operate upon an antecedently determined 'what is said'; but (ii) disambiguation is necessary in order to derive a unique 'what is said' and (iii) pragmatic conversational principles are crucially involved in disambiguation and hence in the determination of 'what is said'. Given this, there will seldom be a sense of 'the meaning of what is said' in which reference to the independent intentions of the speaker in context is not required - or, therefore, any purely linguistic concept [defined as what is specified always and already by the competence grammar] of 'the meaning of what is said'.[68r]

In other words, this "compelling argument" for pragmatic intrusion suggests that the individuating linguistic properties of linguistic expressions are crucially determined by performance factors; that the linguistic is not so much begotten and pre-specified as constructed, and that the linguistic is inseparable from the actualizing cognitive events and procedures.

In the light of these remarks it is illuminating to reprise Read's characterization of the substantive distinction between a realism and an anti-realism:

> Where the realist says that every potential infinity presupposes an actual infinity, the constructivist replies that every potential infinity presupposes an operation and entails that there is no ["antecedently determined"] objective reality. What is true [or existent] is so only in virtue of our ability [a practical/performance-type ability] to apply the operation to a successful outcome, not in virtue of any correspondence with any such [antecedent] reality. ...

> [Where, on the one hand, the constructivist does,] the realist does not take the notion of construction seriously, as imposing [neither] any real constraint [and nor as contributing substantive properties to the constructed result].[69r]

It is also worth mentioning, as a somewhat speculative aside, how such an anti-realist perspective would appear to give some purchase on the phenomenon that Chomsky originally selected as the primary puzzle to be explained, that of creativity/novelty: it would seem to be accountable, at least in part, not by positing an antecedent infinitude, but by the crucial role that context plays in fixing a value for radically underdetermined content. From this property of underspecification there emerges, as observed by Humboldt, the apparent infinitude of natural languages despite their finititude of means:

> the property of underspecification, far from being aberrant, ensures that natural languages are a highly economical vehicle for communication, enabling sequences to be used over and over again with different interpretation in different contexts ...[70r]

Chomsky was indeed correct to indicate that the phenomenon is a matter of, and to be hived off onto, performance, and he was also arguably correct first time round to indicate the centrality of the phenomenon. What comes out of such observations is the suggestion of the explanatory need for a "blurring" of the distinction between competence and performance.[71r]

Such conclusions are resisted by Burton-Roberts. On his account this constitutive role for the actualizing procedures and operations is external to the properly, and resolutely always and already, linguistic. Such a performance component is concerned with the determination of which member of the set of antecedently given linguistic expressions is being represented on any particular occasion, but has no constitutive role in fixing the properties of that expression. The notion of 'representation' that is intended here is that in which to say 'x represents y' is to say that 'x is in a relation to y', where y exists independently of its being represented by x. It is in this sense that a portrait, for example, standardly represents some individual who has an existence independently of being portrayed.

What, however, is "philosophically puzzling" about the relation of representation is that this independent existence need not be the case: "while the existence of a relation between two things trivially entails that they exist, this is not true for the relation of representation."[72r] For example, Botticelli's *The Birth of Venus* represents an event that, I think it is safe to assume, has not taken place, is not real. One might wonder whether that event can be said to have any antecedent existence elsewhere than in the representation. Perhaps this is clearer in the case of depictions which do not have, unlike the Botticelli case, literary or mythic antecedents; a surrealist landscape by Dali, for instance, or non-figurative art in general. The crucial point is that, in this case, unlike in the case of a portrait, there are no means to pick out the individuating properties of what is represented except by reference to the representation itself, the x, *representans*, and the y, *representatum*, of the relation cannot be held apart.

The question that emerges out of the canvassing of these considerations is that of whether the relational sense of representation, the sense required by Burton-Roberts' realism, can be accepted. This returns us to Wittgenstein's remarks (and our kites). It is, seemingly, the case that objective sense can be made of the idea that some y linguistic expression is being pointed to by some concrete, external behaviour. As Burton-Roberts reminds us, when, in some syntax class the teacher asks the students to consider some string that she writes on the board, she is not asking them to consider the graphic marks, but the expression as an abstract linguistic type.[73r] In much the same way the instanced kite might be conceived as a means to represent the ideal kite. But, one might ask, where can we find this ideal kite or sentence, how can we individuate its type, how can we point to it, except through the tokening, a particular concrete and actual event? If we wish to claim some antecedent and independent existence for the type, the *representatum*, then we are faced with the problem of having to claim that it can be represented otherwise, for if something exists independently of its representation, then any particular representation can be exchanged for some other, just as one can pick out some person by means of different portraits. The challenge is similar to one made by Wittgenstein where he asks us to separate words and meaning as if they were in the same relation as are the words to the music of a song.[74r] In other words, how do you specify which linguistic entity you are referring to without the representing tokens, i.e. these particular tokens in this particular context with the corresponding and particular cognitive activity? The two, it would appear, are highly resistant to such separation.[75]

To take a different tack toward the same destination (and compare with our remarks regarding Katz' similar proposal (pg. 106)): suppose we accede to Burton-Roberts' proposal, then we appear to have instituted two areas of study, the linguistic proper - what is represented by behaviours (and also all those unactualized/ unrepresented and indeed, impossible to be represented linguistic expressions) - and a study of the para-linguistic means to represent. Suppose this latter enquiry were to make progress and were able to propose some account, presumably in terms of a set of cognitive procedures and knowledge about representational elements. If this were achieved, if we understood the nature of the actual events, what would be the residue for linguistics proper? Why must it be the case (care of some sort of conceptual necessity, perhaps) that linguistics proper could not be subsumed by this other enquiry, why must there be anything to be explained other than this means to represent, where 'represent' would take on the sense of the conflation of the x and y of the relation?

It is just such a recognition of the inseparability of the constructive, cognitive event from the object created by that event that underlies the constructivist/intuitionist project in mathematics. Whereas mathematics is primarily concerned with the nature of the objects constructed, with, in Carr's terms, a constitutive account (and Wittgenstein reminds us that one can be interested in it in this way), however, linguistics, at least as instigated by Chomsky, has set itself the task of offering an explanation as to why the natural linguistic is as it is, and to attempt this by way of positing an answer in terms of properties of the human mind/brain. A primary burden of our discussion has been to suggest that, on the one hand, in positing a body of declarative, autonomous and internally represented linguistic knowledge, where any temporal/procedural dimension is suppressed, what has been denied is precisely a psychological ontological status and explanation for the linguistic, and, on the other hand, what our present considerations suggest is that the (metaphysical/atemporal) realism that founds this approach, a realism at the level of a set of antecedently given sentences, has founded a study that answers not to the actual linguistic, but to some second order idea of the linguistic, as if this idea, in effect what is articulated by a level one function, were what constitutes the necessary (indeed, conceptually necessary) and antecedent possibility of the actual. What has been answered to, the object of study, has been created, to reprise Saussure's remark, by taking up the atemporal perspective, as articulated by the formal system metaphor, of a metaphysical realism - that "natural impulse", as Putnam reminds us - that lays down rails as if to infinity and rails, it turns out, that lead us awry in an attempt at an

explanation. The suggestion is that what is needed is a perspective that recognizes that, at root, the linguistic is an activity, or, to use a Heideggerian turn of phrase, that the temporal is the horizon of the linguistic: a language is a series "of structures in time ... in which the wholes and parts are events" and to understand its nature we need to understand the nature of those events *qua* events.

4.0 Directions.

Wittgenstein's strictures in *Philosophical Investigations* lead to a species of linguistic anti-realism/nominalism together with a concomitant subtle, but ultimately (and notoriously) anti-explanatory behaviourism. What we are encouraged toward is the recognition of prefiguring rules, i.e. what articulates a metaphysical realist idea/theory of the linguistic, as precisely "phantasms"; as, at best (compare our previous discussion of Searle and Dennett), instrumental fictions. It is this fictionality that Wittgenstein purports to "shew": that we "can adduce only exterior facts about language", that we "must do away with all *explanation* and description must take its place."[76r] As one commentator has put it, "there is nothing to be explained ... it is a pseudo-question."[77]

A further perspective that interpretably emerges, what is "shewn", is that there is no licit meta nor theoretical-perspective on the linguistic to be had; there is no other place to be other than in the (E-)language we use and in its individual uses on individual occasions. In attempting the transcendence of some meta-perspective, in effect the epistemic access of a theory-constitutive metaphor, one is guilty of wrenching words from their "original home", and, it appears, for Wittgenstein there is nowhere else other than this original and normative home: language is only in its use, a set of external events that defy, indeed are falsified by, theorizing.[78] It is, seemingly, the very multiplicity and the unsystematic muddle of the pre-theoretical linguistic, i.e. the point of theoretical (and this thesis') departure, that Wittgenstein emphasizes and takes to be ineliminable: it is as if this is sufficient to confer on individual linguistic objects the status of an unnegotiable and unanalysable uniqueness.

Translating this into an older philosophical idiom (compare our previous remarks, chapter V, footnote 8), this refusal to attempt an explanation amounts to a general distrust of universals (hence the nominalism): universals are posited, as logically and epistemically prior, precisely for the purpose of explaining how it is, and what it is, that we get to recognize in some particular event such that that event is a

particular instance/token of a general type; how it is that different events are recognized as evidencing similarity. The Chomskyan (and Fodorean) rationalist explanatory strategy is to posit that such (re)-cognition of an event as an instance of an x requires that one already has a concept, a mental representation, of an x; to recognize something as an instance of a sentence type, one needs to have that sentence type pre-specified; to learn a language (and its meanings) one has to have the language (and its meanings) always and already: a language is begotten not made.

If, however, we are debarred from positing any universals, in their modern, cognitivist guise as mental representations, then we are seemingly debarred from any explanatory account (for, it will be recalled, these have been taken to be the explanatory *sine qua non*[79r]), and we are reduced to a science of cataloguing. Yet, the reason why universals are posited in the first place, why they appear ineliminable, is that they are required to account for perceived or accredited identity between distinct spatio-temporal events, for example between these following events: i) word ii) **word**. Moreover, it is seemingly a brute fact that, however we are to understand this phenomenon and relate it to properties of the mind/brain, such accredited identity between, and replication of patterns and structures in, distinct linguistic events is the case: linguistic behaviours are not unanalysably unique (and hence unlearnable), there do seem to be some such "phantasms", some constancies across different occasions of use of the same expressions.

To put these remarks in the obverse: if linguistic phenomena are unanalysably unique, this is to say that there is nothing held internally in common between such phenomena other than their being members of a set that is definable only vaguely in extension (some "family resemblance"), not in intension. Credence for such a view requires both an extreme form of scepticism as regards the regularities that linguistic enquiry has repeatedly adduced and a brute, stipulative certainty of the inexplicability of the human language faculty. It is difficult to sufficiently motivate either. On the one hand, the very fertility of the generativist enterprise in demonstrating regularities suggests that there are properly such; on the other hand, one might ask on what might this certainty of the impossibility of an explanation be based other than a stipulation of the normativity of the pre-theoretical, vague muddle and a correlative stipulative disallowance of any reconfiguration of our conceptual access onto the phenomenon. It is our contention that such a disallowance, with its explanatory nihilism, is undermined by even the most cursory inspection of the history of science - such reconfiguration is precisely the manner in which the sciences achieve themselves. It is our further contention that the former of these observations (of the recognition of regularities)

suggests the possibility of some proper causally explanatory account and an account which, if we are to accept one prime regularity - the diversity of human language within parameters that define a similarity - then we need an account which makes reference to some innately prefigured/prefiguring system, but an account that is able to take note of and negotiate the concerns we have raised.

The detail of how such a project might be carried out is beyond the present meta-theoretical remit, however, we may take the concerns we have raised as providing a perspective on an empirical project (Kempson *et al.*'s LDS_{NL} framework) that is presently in the course of development. In the following we shall consider how this project might answer to our concerns. The purpose is not so much to canvas for acceptance of the framework as to illustrate the possibility of developing a linguistic theory that parallels a mathematical constructivism.

4.1 Realizing a linguistic constructivism.

What we interpret our selection of remarks by Wittgenstein to suggest, in pointing us toward the "machine" as opposed to its idea, the "machine-as-symbol", is that a fruitful line of enquiry, one which promises to avoid the breaking of contact we have remarked on and would potentially allow for a *rapprochement* between linguistics and psychology/psycholinguistics (construed as a mind/brain-internal enquiry), would be to examine the extent to which linguistic phenomena are made/constructed, as opposed to simply begotten; that they achieve their individuating properties in the construction process. In other words, this would be to recognize linguistic phenomena, first and foremost (in their first-order phenomenology) as cognitive events; as, in Hockett's phrase, "structures in time" in which the temporal/dynamic aspect is crucially explanatory of their properties.

This would be to reconfigure the domain as constituted by a set of actualizing procedures along with a concomitant emphasis on cognitive tokenings. The object of study comes to be reconfigured as, in Kempson's phrase, "the cognitive architecture that makes language processing possible".[80r] This offers to take us away from generativism's theory-institutional (metaphysical realist) conception of the domain as constituted by a conceptually problematic, always and already infinite set of types and, in so doing, as we will outline below, this reconfiguration promises to relieve us of certain of the attendant conceptual tensions we have indicated.

From this reconfigured perspective, the actualizing construction/performance process is viewed as an integral element of realized/recognized linguistic phenomena, conferring individuating properties on cognitive events. Individuating linguistic properties emerge, (one might say E-merge,) as is suggested by the phenomenon of pragmatic intrusion, in event; a language is not so much something that we know, which knowledge is supplied by some set of always and already mentally represented axioms, but is, rather, something that we do and make on individual occasions. The contention is that it is just such a constructivist perspective, taking the actualizing construction procedure as integral of the object of study, which holds out the hope of an explanation that is both properly grounded in internal properties of the human mind/brain and which renews contact between actual-world linguistic phenomena and the objects of linguists' concerns.

The question posed for an empirical enquiry run along these lines amounts to a question of what, from the perspective of individual psychology (and it must be individual psychology for there are no linguistic events other than those that involve individuals' cognitive acts[81]), constitutes the basis, not restricted to a body of declarative knowledge, for linguistic behaviour. Put in another idiom: what are our commitments; what is to be posited as cognitively real and explicitly specified by some *sui generic* language faculty; what is supplementary to this faculty and what in the results of the activity is epiphenomenal? This is to ask how the gross phenomenon of the linguistic is to be carved/modularized, and how we are to understand the inter-relation of such modularity and its realization in the mind/brain. To put it in yet another way, the issue, and a resolutely empirical issue (i.e. not a matter of some sort of conceptual necessity run off the formal apparatus applied to the task) is one of chickens and eggs, and, recalling Churchland's remarks about nineteenth century embryology, what it is in the egg that is responsible for the actual results.

The LDS_{NL} project is to develop a model of utterance interpretation; a model of on-line, "left to right" (i.e. temporally sequential) processing. The account is expressed through a posited dynamics of how structural properties of interpretation are constructed in the course of processing. Syntax becomes dynamic and, rather than being defined over strings construed as atemporal objects, is defined over an evolving algebra - a series of transitions - of tree descriptions that are the structural basis for interpretation. In this way syntax is emergent out of the parsing process: there is no body of mentally represented axioms constituting a syntactic competence, rather syntax becomes largely epihenomenal (compare chapter 1, footnote 78). This series of

transitions, the dynamics of the system, is articulated in terms of a parsing schema - a set of prespecified (innate and hardwired) combinatorial and structuring operations that take as input (as premises to an inferential sequence) the combinatorial properties (expressed in a type language) of incoming lexical items. It is this on-line sequential input that drives the incremental process of building up structure. The hearer's cognitive task is to construct from the input, as given in its sequence, a formula of type *t*, i.e. a propositional content.

This *sui generic* linguistic system (on the model of a Fodorean input system[82r]) is not (*pace* Fodor) encapsulated from other more general cognitive processes. The notion of modularity that is posited is one of an interactive or integrative modularity whereby, for instance, more general central processor information can be accessed on-line to the purpose of fixing underdetermined lexical semantic content (the central processor is assumed to run on relevance-theoretic, most cognitive gain for least effort principles). This penetration of the linguistic system is circumscribed (i.e. its not a Liberty Hall of free inferential effect) by virtue of the hearer being committed to building up a structure by way of incorporating all the information (i.e. combinatorial requirements) encoded by the given words in their particular sequence. What is encoded and *sui generic* is not defeasible and the structure-building process is monotonically incremental (i.e. the hearer cannot, in the course of processing, undo what is already done). A judgement of acceptability comes to be construed, (hence providing a rationale for the use of speaker judgements as relevant data,) as recognition of a sequence as providing information, as presented by the on-line series of words, such that all that information is incrementally incorporated, in the sequence in which it is given, into the process of deriving a structure of which the interpretation has at least one complete propositional content. Where that information has to be supplemented, or where the incorporation of the information requires considerable processing effort, then acceptability will tail off, on a relevance-theoretic story, in proportion to the extent that processing cost does not achieve adequate cognitive pay off.[83] Although the theory takes on the hearer's perspective, in its light the obverse task for a speaker may be figured as one of packaging, through a selection of a sequence of lexical items, a program (or a cumulative series of sub-programs) that directs the construction of the intended propositional content.

The overall result is that the human linguistic faculty becomes integrated within, and to a certain extent subsumed by, a theory of processing: the human language capacity is to be accounted for in terms of the cognitive architecture that

makes language processing possible. It is not that performance is a supplement in the contingent/extraneous sense of "supplement", but is the constructive condition for the human language faculty (compare chapter I, footnote 82). This cognitive architecture is held to be innate and hard-wired; language acquisition comes to be identified with the acquisition of the relation between phonologically individuated lexical items and some conceptual content (also deemed to be innate *à la* Fodor and of which the combinatorial properties, i.e. the types, are a reflex[84]) together with possible idiosyncratic information concerning how each item provides directions to the parsing procedure.

In the light of this sketch we are able indicate how it offers to address certain of the concerns we have raised.

In postulating a hard-wired parsing schema that characterizes a set of procedures, something that, presumably, neurological structures have evolved to do, there is an explicit reduction in commitment to explanatory constructs that are individuated intensionally and to which an individual is in a relation of tacit knowledge:

> there are no grounds on this view for seeing control of language as involving in any sense a set of axioms to which the language user must stand in a propositional attitude of knowing.[85r]

The remaining such commitment is, firstly, to knowledge of lexical items in their pairing of a phonological realization and a concept. It is to be noted that this passes Searle's criterion of admissibility for intensionally individuated objects "as that which is potentially conscious".[86r] For it is the case that such knowledge can be, to a significant extent, consciously retrieved. This leaves somewhat problematic the status of the procedural (directions for use) content that a lexical item provides to the parsing process. Kempson *et al.* make an explicit disclaimer as to the retrievability of such "knowledge". However, one might be tempted to stretch Searle's notion of being "potentially available to consciousness" by remarking that, interpretably, combinatorial properties of lexical items are retrievable in the sense that they emerge into consciousness in linguistic behaviour. It is the basis for cloze exercises in language teaching: fill in the gap, and how do you know, and bring to consciousness, how to fill in the gap in that way? For example: i) Tom kicked _____. ii) Tom put the book _____. iii) Although it was raining, _____ .[87] Furthermore, there is a body of

psycholinguistic evidence that is consonant with the notion of lexical items "projecting" such combinatorial requirements. In a study conducted by Wright and Garrett (1984), their experimental findings suggested that word recognition time decreases where a lexeme of a specific syntactic type is required (or "projected") by a syntactic context and where that expectation is met. A further experiment by Gorrell (1987) duplicated the results in respect of ambiguous lexical items. Here the findings were consistent with projections being made for each possible reading of an ambiguous lexeme: word recognition time was decreased where a subsequent word, consistent with either projection, was encountered in parsing. A subsequent word that is not projected for, although a possible grammatical continuation, showed an increase in recognition time.

In citing such psycholinguistic studies, in their being deemed relevant at all, there is an implicit comment on the canvassed promise of a *rapprochement* between linguistic theory and the brain sciences. In other words, in positing a linguistic theory that takes the cognitive/processing architecture as the primary object of study the possibility arises of relieving linguistic theory of its questionably principled empirical strandedness from other (e.g. psycholinguistic/neuro-scientific) theoretic perspectives and the instrumentalist interpretation that this strandedness forces. To put this another way: once one posits this object of study, (i.e. the cognitive, processing architecture,) then one diminishes the possible grounds one might have to argue for the irrelevance of other brain sciences to the task of theory (dis-)confirmation. Of course, what also accrues from the explicit relevance of such other perspectives is the prospect of achieving some other linguistic-theory-external epistemic access, some other "spectacles" and, consequently, some shorting of the "closed-circuit functioning" that we have suggested has been the condition of linguistic theorizing in the generativist paradigm.

It is to be pointed out that such a *rapprochement* with psycholinguistics has not been investigated in the development of the LDS_{NL} framework. There is a related and unaddressed question as to how the inferential dynamics of the parsing process, as characterized by the parsing schema, maps onto real-time processing dynamics (compare Pylyshyn's remarks cited on pg. 162). However, what is provided by the characterization of a parsing schema is a level of description that, interpretably, coincides with Peacocke's requirement for a psychologically real theory to be pitched at, in terms of Marr's hierarchy, level 1.5, in which is specified "the information drawn upon by the relevant mechanisms or algorithms in [the] subject."[88r] (compare pgs. 162 ff.)

It will be recalled that what brought to question the psychological ontological status of generativist theories of competence was their taking as an object of study a level one function. We argued that such objects require to be taken as having either a Platonic or autonomous ontological status for the reason that, on the one hand, they exceed, in their generative capacity, the necessary finititude of any possible concrete realization, hence leading to the uncomfortable position of having to negotiate the notion of the possibility of impossible to be actualized objects. On the other hand, the abstraction of a level one description makes the properties of any instantiating medium irrelevant to their explanation: they are multiply instantiable. Given this property, the criteria of identity for any behaviour conforming to the level one description cannot contentfully advert to any internal (e.g. mind/brain-internal) properties. What this means is that an explanation of behaviours pitched at level one carries no commitment to there being any commonality among the internal states of minds/brains that instantiate that function. This is why studies of those states are irrelevant to the falsification of posited level one functions. However, it is the case that there is precisely evidence for such commonalities/regularities among the mind/brain-internal states of language users - its what makes psycholinguistics, and the neurosciences in general, more than a mere random catalogue of experimental findings. Put another way: it would be hugely surprising if such regularities were not the case. It would be equally surprising if such regularities were not constructive of the nature of natural languages, i.e. irrelevant to an explanation of why natural languages are as they are. Precisely what a level one account insulates us from is such an explanation and what it defers is the commensurability of the constructs of the linguistic and neuro-scientific domains.[89] What an account pitched at level 1.5 goes some way toward addressing is that of the realization relation between an individual and a natural language in terms of properties of the internal architecture of the mind/brain.

An objection (compare Burton-Roberts remarks cited pg. 243) that might be raised to an account run along these lines, lines more or less parallel to constructivism in mathematics, (and an objection that runs more or less parallel to standard objections to mathematical constructivism,) is that we appear to lose the requisite generality of the theory by failing to answer to the notion of "any sentence of a language", the notion that has constituted generativism's pre-theoretical given (compare J. Katz (1996, pg. 281)) and to which any adequate linguistic theory must seemingly address itself.[90] It is just this given which, articulated as a set of abstract types, (again compare Katz) we "know" is required to be answered to (indeed, it is

such an object that is implicit, even if hopelessly vague, in the everyday idea of "English" or whatever). But if this is the case, then what a theory as outlined above does not answer to is just this set of types, of which the members are fully individuated by having their full complement of linguistic properties in respect of the syntactic, semantic, phonological *et al.* tuple. This is because, if all such properties are only fixed in respect of a particular context, i.e. in the constructive procedure with its attendant vagaries, (care of the feeding of central processing and context-relative information into this procedure), then the only linguistically interesting, fully linguistically individuated objects are the individual cognitive (and tokening) events. We would seem to be returned to something like a study of corpora from which the Chomskyan paradigm advantageously departed in the fifties, and what was advantageous was just the attainment of this requisite generality.[91] Putting this in the obverse, if some constructivist account on these lines is in broad terms correct, then natural languages, as understood by the generativist paradigm, i.e. as the entities which constitute the posited, always and already object of study, do not exist. At least, their existence, in their generality, is no other than a feigned and phantasmic existence: we end up being anti-realist about languages.

A question then is one of how to readmit/reconfigure some requisite level of generality such that the enquiry is any more than a taxonomy; in other words how are we to reconfigure the notion of types, which notion is what has articulated this generality and enabled us to pass beyond a set of particular events?

Burton-Roberts, it may be recalled, remarks that when, in a syntax class, the teacher tokens a string on the board, she asks the students to consider, not the chalk marks, but the type that those chalk marks token. However, this abstract object is only available to be considered on condition that we can construct some object from our processing of the marks of chalk (what Burton-Roberts figures as representing some antecedently and independently existing object, i.e. the type). But, as argued, we do not, and cannot, get to identify the type without, or otherwise than through, this cognitive activity. And, we further suggested, that grounds for questioning the antecedent, independent existence of this object is that it cannot be identified otherwise than through this cognitive act, which seems to be antecedent to, or at least the condition on, recognizing the type. Moreover, what the phenomenon of pragmatic intrusion implies is that it is through this, and only through this context-dependent and temporal event that the fully individuating properties of the object are constructed. This might suggest an option on construing a type not as antecedent, but posterior to the cognitive event. (It might also be pointed out that this act of contemplation, by

which we get to consider the type, is not obviously a first-order linguistic activity, but is, rather, a derived and artificial form of behaviour: one does not, in the ordinary run of things, contemplate linguistic behaviours at all, i.e. take on a theoretical perspective, one simply understands them.)

Given these remarks, types come to appear as the shadows cast by some constructed objects illuminated by a certain light. They are, to clip the wings of poesy somewhat, the *abstracta* of some second-order thinking about first-order, cognitive phenomena. To reprise Burton-Roberts' phrase again, types are not of language but are "of-and-for-the-linguist": types are not themselves linguistic entities but are the projections of our second-order/theoretical thinking about language; they are the abstract projections of theories.[92r]

In the light of this we can revivify the antecedence of types, and the requisite generality of the enquiry (i.e. we get to talk about the non-actualized) exactly by construing types as abstract projections of the linguist's theory; that is as predictions made by a theory that constructs and articulates an abstract, multi-dimensional categorial space (some tuple of properties, where each property might be figured as constituting an axis on a multi-dimensional graph) that actual events either conform to by fitting a point in the space of this graph (hence corroborating the theory), or do not conform to (hence offering disconfirmation of the theory.) This abstract space will include predictions (i.e. types as *abstracta*, points in the graph) that have yet to be instantiated; in other words, we get to achieve, through our theoretical apparatus, a generality such as the enquiry is not restricted to corpora. We also get a theory that is potentially (virtually) true, not by being duplicated by a set of axioms (a theory) mentally represented in the minds/brains of speaker/hearers (and which set gets us to know always and already the set of sentences of our language), but potentially true because the cognitive, actualizing architecture constitutes a nomological domain (or set of interacting nomological domains: it is, by hypothesis, an integrated modular system) that is characterized and delineated by some statement of the interacting laws of those domains.

Furthermore, one of the relevant parameters, one of the constraining factors on the categorial space projected by theory, will be the physical limitations of the neuro-physical substrate. What this amounts to is, in Russell's phrase, the relevance of "the merely medical" contingencies of the cognitive architecture. But these are not, from what we know, contingent, for they are the very condition for there being natural languages at all. What we have is a means to embody the linguistic and a perspective that takes cognizance of the actualizing machine. What accrues is that we

come to negotiate the impossibility of possible strings that are licensed by generativist theories: they are outside of the categorial space of the modularly integrated and embodied theory. In short, we achieve an account for a neglected but obvious fact: that of the actual finitude of natural languages from the perspective of individual psychology, and with this comes a concomitant *rapprochement* between linguistic theory and actual linguistic behaviours. What we get, from this perspective, is no real, first-order existence for language apart from the individual cognitive events as constrained by the cognitive architecture that makes actual language possible.

Finally, what we also get is, to rework a phrase of Chomsky's,[93r] an objective and autonomous (in Carr's sense), second-order existence for language in the theories that articulate the phenomenon. Consonant with the internal realist subtext running throughout our discussion, theory is not the means to transparent access onto the world such that a theory is to be conflated with the world in itself, but it is one more, and ontologically distinct, object by means of which we attempt to understand and negotiate our being in the world.

Footnotes.

[1] L. Wittgenstein (1958, section 108).

[2] W. von Humboldt (1970, pg. 27).

[3] C. Hockett (1977, pgs. 110 and 108), cited in C. Hutton, (1990, pg. 143).

[4] N. Chomsky, (1957, pg. 18). It has been suggested (e.g. R. Harris, 1980) that the conceptualization that underlies much thinking about (provides epistemic access onto) the linguistic is that of language as writing: a not inaccurate definition of a book, i.e. a collection of writings, is that it is precisely a set of always and already sentences. A similar theme, (that thinking on language has invariably taken, and perhaps needs, writing as its originary metaphor and means of conceptualization,) runs through Derrida's *Of Grammatology*.

[5] C. Pollard and I. Sag, (1994, pg. 14).

[6] Hutton argues (1990) that type/token thinking, to the extent that it pervades linguistic thought, is a primary obstacle to the furnishing of an explanatory linguistics.

[7] The phrase is Burton-Roberts' (1994, pg. 188). The points raised in the following rehearse Burton-Roberts' observations.

[8] N. Burton-Roberts (1994, pg. 189).

[9] Burton-Roberts' paper does the service of taking these problems seriously and attempting a solution that preserves the generativist's given object of study and its correlative realism. The problem of the resistance of types to having constituent parts (hence linguistic properties apparently having to be predicated of only tokens) is dealt with by a "Principle of Type-Token Affinity" (pgs. 189 - 191). The proposal (compare Bar-Hillel, (1970, pg. 367) is to distinguish between types that have physical tokens, and types (e.g. linguistic types) that have tokens/occurrences that are themselves abstract. These abstract tokens, properly and in keeping with actual practice, are primary objects of study and are of the same ontological category as their types and so, purportedly, unproblematically constituent parts of their types. This solution, however, comes with a "health warning" because it disallows the type/token distinction from attempting (as we have seen, illicitly attempting) what it has been asked to do, namely articulate the relation between the linguist's abstract objects of study and concrete actualizations. We still have the problem, as we outline, of the solipsism of the linguistic. We take up Burton-Roberts' addressing of this issue below.

[10] C. Hutton, (1990, pg. 168).

[11] T. Moore and C. Carling, (1987, pg. 16). The original has "our everyday notion of a sentence". I do not see why we have to take such a notion as normative - compare our mistaken everyday notion of a static earth; do we not say that it is the sun that rises and sets.

[12] It is worth pointing out that Burton-Roberts' abstract tokens (see footnote 9) are supposed to be consonant with a psychologism, they are, we are informed, "cognitive". In essence, our following remarks are to the purpose of disabusing us of such an ontological status.

[13] J. Katz, (1996, pg. 271). It is interesting to note the whiff of Galileanism here, as if the linguistic *has to be* explicated in terms of a grammar, i.e. a mathematical apparatus. It also seems, correlatively, to be the case that Katz is privileged in knowing in advance of enquiry (presumably by his Platonist's intuition) quite what it is that is to be accounted for: a remarkable gift. Of course, one has in advance of enquiry some concept of a language, but enquiry is not defined by, and constrained to, having to explicate that concept.

[14] J. Katz, (1996, pgs. 278 - 280). The bold italics are mine and are there to encourage the reader to ask a question, one to which we shall return, about the source of this "knowledge". (Here's a clue: if the linguistic is "known" to be denumerably infinite, then our access to this knowledge cannot be from experience. So, how do we know of this infinitude, except on the basis of the discourse that follows from the epistemic access provided by some theoretical apparatus (e.g. a generative grammar, a device which packs infinitude) applied to the task of understanding the linguistic? How can we ascertain whether it is the linguistic that is indeed infinite or whether the infinitude is no more than what is put there/projected (and unfalsifiably) - a skeuomorph - by the apparatus which provides our epistemic access, and which, perhaps, to reprise Saussure, "creates the object of study"? This point is not restricted to just the question of the infinitude of natural languages, but to the whole notion, the founding generative assumption, of an always and already, given set of sentences that constitutes the object of enquiry.)

[15] N. Chomsky, (1980, pg. 34).

[16] J. Katz, (1996, pg. 280).

[17] N. Chomsky, (1982, pg. 16).

[18] J. Katz, (1996, pg. 281).

[19] The phrase is Kempson's (e.g. 1996, pg. 594).

[20] The Keplerian example we cited is illustrative: his suggestion is that "we have no right to assert that a regular heptagon exists until someone has prescribed the geometrical method by which it can be constructed with a ruler and a pair of compasses." (J. Barrow, 1993, pg. 190).

[21] S. Read, (1995, pg. 215).

[22] J. Katz, (1996, pg. 281).

[23] A. Markov (1954), quoted in J. Barrow, (1993, pg. 221).

[24] Katz (1996, pg. 281, fn. 7) makes the same observation and goes on to remark that Chomsky is quite right to insist that length is no determinant of grammaticality because "it runs contrary to the deeply structuralist character of syntax." One might also note that it runs contrary to a literal rendering

of the formal system metaphor out of which, arguably, the infinitude of the natural linguistic is derived in the first place.

[25] N. Chomsky, (1991, pg. 19).

[26] It is this verification-transcendence which is a primary complaint against realism. The objection is to the idea that a sentence (or proposition) is determinately grammatical (or true) independently (always and already) of it being recognized or actualized.

[27] L. Wittgenstein, (1958, section 120).

[28] *Ibid.* section 104.

[29] N. Smith and D. Wilson (1979, pg. 84). A more sophisticated exemplar of this form of reasoning is Postal's and Langendoen's argument (1985, see chapter III, footnote 27).

[30] N. Smith and D. Wilson, (1979, pgs. 48 - 49). The same reasoning is used by Newmeyer, and the same objections may be raised. (One may also note that his reasoning cites considerations of "elegance", as if this were necessarily pertinent to a contingent and evolved faculty.):

> It makes sense to postulate the grammaticality of certain unacceptable sentences (...), since the most elegant account of their ill-formedness involves the interaction of general linguistic principles with principles from outside the domain of grammar proper. (F. Newmeyer, (1983, pg. 52)).

[31] L. Wittgenstein, (1958, section 218).

[32] Carr makes a similar point (P. Carr, 1990, pg. 43).

It might be noted that the considerations canvassed rehearse the central complaints of the anti-realist regarding verification transcendence and are parallel to those which Dummett adduces in objection to the idea that meaning is to be accounted for in terms of a realist notion of truth. (See M. Dummett, (1976).) Dummett's central contention is that a realist (always and already) notion of truth places many sentences (e.g. counterfactuals) beyond our capacity to recognize whether they are true or false. (And if the realist notion of truth is supposed to be doing the business of explicating meaning, then such verification transcendent sentences, those beyond our capacity to recognize them as either true or false, must be meaningless, and they are not.) Similarly, a realist view of the generative capacity of a grammar, with its infinitude, places many sentences beyond our capacity to recognize them as grammatical, the grammaticality of such sentences becomes a transcendent fact. But how can this be, for what constitutes such transcendent facts is the I-language, i.e. what we *know*, so we are in the peculiar position of knowing a language and not knowing it at the same time. Indeed, Dummett's complaints recapitulate those of Hutton and of Moore and Carling and Burton-Roberts' observations: a realism articulated through an I-language fails to account for a connection between knowledge and use.

[33] This example is Carr's (P. Carr, 1990, pg. 43).

[34] Garden path sentences are sentences that, while they are grammatical, systematically give rise to judgements of unacceptability. Bever's canonical example being: "The horse raced past the barn fell". For a catalogue of types of garden path sentences and further detail see B. Pritchett, (1992). At the heart of the phenomenon is ambiguity, either lexical or structural.

[35] J. Higginbotham, (1987, pg. 128). Presumably the clerk consults a ledger cataloguing all the always and already members of the set of sentences of a language. Notice again that the purchase on the linguistic is through a metaphor of language as writing.

[36] For example, (N. Chomsky, 1980, pg. 221):

> A sentence that is incomprehensible in speech may be intelligible if repeated several times or presented on the written page, where memory limitations are less severe. ... [In this case,] the same knowledge can be applied with fewer extrinsic constraints.

[37] See our comments below (pg. 234 ff.). For a related and highly elaborated view of intuitions as reflecting knowledge of intersubjective norms, see E. Itkonen (1978), reported in P. Carr (1987 and 1990). See also C. Snow and G. Meijer (1977).

[38] This is Garrett's observation quoted as a prefatory remark in Fodor, (1983).

[39] N. Chomsky, (1991, pg. 19).

[40] N. Chomsky, (1986, pg. 36).

[41] C. Wright, (1986, pgs. 204 - 238).

[42] J. Higginbotham, (forthcoming).

[43] Of course, a principle of bivalence is what comes out very naturally from a common and prescriptive idea about language: the idea that makes sense of talk about "English" or "Italian" or whatever. In other words, the notion of a language as described and given always and already by, for example, a list of rules of proper usage, a view which is beloved by pedants everywhere and is part and parcel of everyday (metaphysical realist) thinking about language.

[44] Chomsky alludes to such a performance orientated perspective in remarking that:

> It is sometimes argued that the language (or "grammar") should be *identified* with the parser, ... Or it is argued that since all evidence comes from performance, we have no grounds for interpreting the evidence as being about anything *but* a parser, which would be much like the argument that since all evidence comes from experiment and observation, we have no grounds for interpreting it as bearing on the systems of the world under investigation. (N. Chomsky, 1991a, pg. 19)

However, the question which is being begged here, is that of whether there is (or is it that, care of some conceptual necessity, there *must* be?) such a, i.e. just *this*, "system of the world". For, after all, surely a set of processing mechanisms count just as much as world objects.

[45] For experimental findings regarding the effect of context on judgements (and, correlatively, on the determining of syntactic structure) see, for example, Crain and Steedman (1985) and Altmann *et al.* (1992). For similar findings in relation to ambiguity in general, see Marslen-Wilson and Tyler (1987). See also our previous discussion of centre-embeddings and the apparent intrusion of competence-extrinsic factors into the fixing of syntactic structure (pg. 231).

[46] L. Wittgenstein, (1958, section 101).

[47] Amongst such artefacts and institutions or, in Durkheim's phrase, "collective representations", one may include "social traits, customs, legal systems [and] languages". These are (compare Popper's mind-autonomous world three objects) "said to 'exist outside the individual consciousness'" (quoted from the entry for Durkheim in T. Honderich (ed.), 1995).

[48] J. Katz, (1981, pg. 79).

[49] P. Carr, (1990, pg. 44).

[50] A reminder of the need for precisely a perspective from individual psychology, and that linguistics cannot be subsumed by the sociological, is, for salient example, the attested facts of creolization. The point here is that in the process of creolization (e.g. in the case of Hawaiian Creole English, (D. Bickerton, 1981 and 1982)) there appear, as if spontaneously, grammatical features that are not found in any of the contact languages. Even more so than in the case of first language acquisition, a socio-linguistic dimension is precluded from providing any explanation. The only available explanatory option, and more generally the only way to explain the diversity of human languages within parameters defining a similarity, is to point towards some genetically pre-specified cognitive properties.

For further reminders of why there seemingly has to be, if we are interested in explanation, a perspective on linguistic phenomena from the brain sciences, see chapter III, 3.0.

[51] For the source, see chapter I, footnote 88.

[52] An illustrative and parallel commentary on the above issues is provided by an excursion into some recent philosophy of mathematics. Hersh (1998, pg. 22) argues that "mathematical objects are a distinct variety of social-historic objects." In doing so he rejects as "futile" Kant's question (corresponding to a founding question of Chomsky's generative linguistics) as to "how mathematics [is] possible." This rejection goes by way of a variation on the theme of epistemic boundedness - "why should the question have an answer?" (pg. 21) - together with the contention that there is no need to look "beyond [mathematics'] social-historic-cultural meaning". Its reality is grounded in its being "a form of life" (overtones of Wittgenstein). Further questions as to how it is to be explained - perhaps through some cognitive story - are another and only contingently relevant country. One might usefully compare this notion of "social-historic objects" with Dawkins' "memes" (1976 and 1989), i.e. cultural objects/ideas such as beliefs or fashions that can be replicated and in the process evolve. In the course of transmission they take on an existence, and constitute a nomological/

ontological domain, that is autonomous of the biological/psychological systems that instantiate them. (Also compare Popper's world three.)

Dehaene (1998) offers a not incompatible two-level perspective, the difference being that he is interested in explanation for its own sake, not for what it might add or not add to the mathematical enterprise. The picture we are encouraged toward is one of a foundational, innate mathematics faculty, a "number sense", which is supplemented by a meta and, in some considerable part, socio-cultural-historic "evolutionary" process of selection and refinement of the mathematicians' results. (In this evolutionary idea Hersh explicitly concurs. Again here the notion of a meme provides some useful conceptual access.) In this sense mathematics is artefactual; one might consider mathematics as rather like a book of quotations of best work which gets added to and deleted from over time.

What our preceding remarks have suggested is the possibility of the appropriacy of a similar two-level perspective on linguistic phenomena. However, in the linguistic case, our contention would be that the meta, socio-cultural level is relatively peripheral. This is for the standard reasons and which reasons distinguish linguistic from mathematical competence: languages, unlike mathematical ability, are common, barring cases of pathology, to all members of our species. Mathematics for the most part (i.e. beyond what Dehaene identifies as an innate "number sense") is something that is consciously learnt; a language is what one unconsciously acquires. Mathematics is mostly socio-historic superstructure, a set of results upon which results, mediated by some second-order reflection, further mathematical activity is based; linguistic behaviour is, in contrast, predominantly first-order behaviour: the expression of the psychologically instantiated and innate substructure.

[53] N. Burton-Roberts, (1994, pg. 196).

[54] It is worth noting that Wittgenstein's return to philosophy in the late twenties was prompted, reportedly, by his attending a lecture by Brouwer (the expositor of the intuitionist/constructivist project for mathematics).

[55] L. Wittgenstein, (1958, section 193).

[56] *Ibid.* section 218.

[57] H. Putnam, (1989, pg. 214).

[58] L. Wittgenstein, (*ibid.*, section 194).

[59] *Ibid.*, section 108.

[60] *Ibid.*, section 109.

[61] L. Stebbing, (1935, pg. 9, cited in C. Hutton, 1990, pg. 33). In this one is reminded of Wittgenstein's famous challenge to "mean a sentence ... without saying [i.e. tokening] anything" (1958a, pg. 37).

[62] L. Wittgenstein, (1961, 4.064).

[63] E. Lemmon (1966)

[64] N. Burton-Roberts, (1994). More recently, in a talk at SOAS (November, 1997) this has been re-termed a "conjecture".

[65] *Ibid.* pg. 192.

[66] *Ibid.* pg. 183 -184.

[67] Wilson and Sperber, (1981). The syntactic correlate of such ambiguity, where performance factors are crucial in determining the individuating properties, would be a string such as "I saw a man with a telescope" and garden paths in general (see footnote 45).

[68] N. Burton-Roberts, (*ibid.* pg. 181).

[69] S. Read, (1995, pg. 215).

[70] R. Kempson *et al.* (forthcoming, chapter 8).

[71] The phrase is Kempson's (1996).

[72] This is quoted from the entry for "representation" in T. Honderich (ed.), (1995).

[73] N. Burton-Roberts, (*ibid.* pg. 188).

[74] L. Wittgenstein, (1958a, pg. 37).

[75] Speculatively: one might say that such separation is possible in the sense that the point of, and what allows for the possibility of, translation between two languages is that some individual proposition (in a Language of Thought?) can be picked out by linguistically distinct forms. Similarly, in reading or revising a text one may note repetition, but one finds that what is identical between two passages is not the linguistic form but the propositional content. In this light, the psychologically realized language faculty appears in the guise of a meta-level, prosthetic procedural device, for the purpose of mapping (or directing a hearer) from a signal to a propositional content. (This is essentially the line taken by Kempson *et al.* (forthcoming)).

[76] L. Wittgenstein, (1958, section 120 and section 109).

[77] J. Russell, (1987, pg. 226). Russell credits such a "Roundhead" interpretation to Baker and Hacker (1984). Chomsky notes (1980, pg. 73) Hunter (1973) as a subscriber to similar views. There are other commentators, however, who disagree with this reading of Wittgenstein (e.g. A. Kenny, (1973, pg. 146)). Compare these remarks with Hersh's position, in respect of mathematics, as discussed in footnote 52.

[78] It is this normative home (compare Husserl's notion of a *lebenswelt* (chapter V, footnote 24)) which is the basis for Wittgenstein's anti-scientism (see C. Chihara and J. A. Fodor, (1967)). I suspect that, on similar grounds, Wittgenstein would look askance at the veritable cottage industry, of which, I suppose, this is an example, that involves itself with attempting to inform us, from some transcendent and systematic perspective, what Wittgenstein *really* meant. Perhaps the most sympathetic and accurate commentator would simply "shew" us by pointing to the books and asking us to read. (Its not very career-enhancing though.)

[79] See Chomsky's citation (1986, pg. 257) of remarks by Demopoulos and Matthews (and which we quoted on pg. 179).

[80] R. Kempson *et al.* (forthcoming).

[81] There is, of course, also thinking and generalizing about those individual acts, i.e. such as gives rise to meta-level, second-order objects, the sort of socio-cultural objects that would have claim to constitute objects of study that are required to be understood as autonomous of individual minds, as being, depending on one's taste in terms, amongst Dawkins' memes or denizens of Popper's world three.

[82] See J. A. Fodor, (1983).

[83] What may be noted here is that acceptability is a case of incorporating all the on-line encoded information, but it is not a case of incorporating all and *only* that information. If there is an integrated feeding relation between non-specifically linguistic systems and the core linguistic system; if realized structures and their individuating properties are not fully determined by this *sui generic* linguistic system, then there would seem to be plenty of scope for positing properties of the core linguistic system that are resistant to falsification. Where predictions of (un-)acceptability are not borne out it is always available to point towards processing difficulties or processing strategies to make good the discrepancy. In other words, we are still potentially in the bind of "closed-circuit functioning" in which there is a non-disconfirmatory dialectic relation between the linguistic system and central processing. We are still in need of other spectacles and other theory-external perspectives.

A further observation to be made is that, if the performance system is that whereby linguistic objects are constructed and take on their individuating linguistic properties, i.e. they cognitively E-merge in cognitive event, and in which events the vagaries of context and central processing are crucially relevant to their construction, then there is questionable room for a linguistically interesting notion of grammaticality/well-formedness, defined over the core linguistic system, that stands discrete from individual judgements of acceptability. This is because to get one it is necessary to abstract/idealize on an axis (of *sui generic* competence) that leaves behind certain linguistically crucial (performance-consigned) factors, i.e. certain context/performance-dependent, individuating properties. Indeed, the linguistically crucial role of such vagaries questions the appropriacy of a principle of bivalence applied to linguistic objects, i.e. the metaphysical realist view that some string is determinately, irrespective of being constructed or constructable, in or out of a language. It would seem that if we want to consider the linguistic domain in terms of a set of sentences then we need a fuzzy set with fuzzy members.

[84] Quite what the nature of this content is is left as "relatively unexplored". However, a suggestion is that items of the mental lexicon, rather than having fixed conceptual content that is constant and unsupplemented in context, are to be better understood as points of storage of incomplete concepts that act as the means to construct specific concepts relative to a context of use. (One is brought to

mind of Wittgenstein's observation of family resemblance between category members.) The picture is once again of the cognitive E-mergence of linguistic properties.

For a similar proposal (in respect of first language acquisition) regarding a correlation between syntactic/combinatorial categories and semantic classes, see J. Grimshaw (1981) and J. Macnamara (1982).

[85] R. Kempson *et al.* (forthcoming).

[86] J. Searle, (1992, pg. 152).

[87] An observation of this ability, (which, at least as far as language teaching is concerned, is indicative of, to the point of testing, knowledge of a language,) was made by D. Milward at a talk at SOAS in May, 1996.

[88] C. Peacocke, (1989, pg. 114).

[89] Churchland and Sejnowski (1990, pg. 230) make a similar observation: in addressing the contention that multiple instantiability constitutes a principled obstacle to the relevance of the neuro-sciences and a reduction of cognitive domains to the neuro-logical they remark:

> So what? If, in any given species, we can show that particular functional states are identical to specific neuronal configurations ... that will be sufficient to declare a reduction relative to that domain.

[90] Compare with an argument, rehearsed by Luntley (1999, pg. 171), to the effect that, if we assume the anti-realist principle that a proposition is true on condition that it is knowably true, then the unsettling upshot is that the only truths are those that are now known. In other words, we cannot pass beyond what is actual.

[91] To make the point in another way (compare footnote 83): a linguistics conducted on the basis of the sketched theory becomes problematically predictive. There seems to be no way (because there seems no way to predict limits on the inferential effects that central processing might induce) to predict, for example, the fully realized properties of some string. It is however possible to place, i.e. predict, parameters on the range of structures constructed, because the claim is that the parsing process is constrained (by the parsing schema that constitutes the cognitive architecture) to a limited set of possible transitions given the sequence of lexical items with their indefeasible combinatorial requirements. In this way we can get a notion of well-formedness and, indeed a level of always and already generality (a set of licensed strings) but only along one dimension (one of the tuple) of linguistic properties, ie. along the combinatorial axis.

We can even, if we wish, get to conjure with a homely, because familiar, infinitude of well-formed strings by "abstracting out all considerations of parsing effort, difficulty of retrieval, or how derivations might compete in the face of more than one alternative" (Kempson *et al.*, forthcoming). But this is, on the one hand, to generate a limitless number of types, and, at the same time, to abstract

out of the very condition for there being languages all, i.e. the rest of the linguistically non-contingent cognitive architecture that makes language possible. In other words, it is to hypostasize a ghost in the machine, and one which, as ghost, is not constrained by the physical limits of the machine (those that define, in Russell's phrase a "purely medical impossibility"). But it is precisely these "medical" limits that, by hypothesis, constrain and provide certain constitutive properties of actual, realized linguistic behaviours. This is why there is no work to do for a notion of grammaticality/well-formedness that does not incorporate these other, actual-language-relevant factors.

[92] Compare this suggestion with Bromberger (1989).

[93] "Language has no objective existence apart from its mental representation." (1972, pg. 169).

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